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MEDICAL JURISPRUDENCE.

PRACTICAL COMMENTARIES ON

DR. CHRISTISON'S PROCESSES FOR DETECTING POISONS.

ARSENIC.

As we are extremely anxious to render our notice of the arsenical poisons as practically interesting and useful as possible, we shall, in the first place, present a condensed but complete epitome of Dr. Christison's excellent history of the chemistry of arsenic and its preparations.* We earnestly solicit such of our readers as are not well versed in the subject, and who wish to become competent for the prosecution of inquiries in this department of analysis, to attend particularly to this preliminary abstract, before they proceed to the processes recommended for the detection of the poison. We would request them also to banish the word "tests" from their memories; this word has produced much mischief, by leading the minds of the inexperienced to place implicit and unquestioning faith on certain reagents, without previously studying the changes and decompositions which they produce. A specific or oracular virtue has thus been unfortunately attributed to them, and errors have consequently long remained latent, which, under a more liberal investigation, would have been quickly detected.

ARSENIC, in the proper acceptation of the term, is a metal of an iron-gray colour and crystalline texture; it has a strong affinity for oxygen, which it abstracts rapidly from the air, from water, or other oxygenized fluids, forming two acids, the arsenious and the arsenic. When exposed to heat, the metal sublimes at a temperature below the red heat of glass, emitting an alliaceous smell; if the sublimation be performed in an open vessel, the metallic vapour abstracts oxygen from the air of the apparatus, and white vapours of arsenious acid are produced, which condense on the sides of the vessel in brilliant octohedral crystals.

The principal compounds which are formed by arsenic, are the arsenious acid or white oxide of arsenic, the arsenite of copper or mineral green, the arsenite of silver, the arsenite of potash, the arsenic acid, the arseniate of potass, the yellow sulphuret or orpiment, the red sulphuret or realgar, and the impure sulphuret termed king's yellow; there is moreover a black compound termed fly-powder, little known in this country, composed of the metal and arsenious acid. To each of these, with the exception of the last, we shall apply a few observations in detail.

The arsenious acid, when newly prepared, exists in the form of white transparent, vitreous lumps, which gradually become opaque by keeping. It is usually sold as a white powder; when heated to 380° Fahr., it is sublimed, and condenses unchanged in minute octahedres. The taste of arsenic has been disputed, but Dr. Christison inclines to the belief that it is entirely insipid, and that the peculiar taste sometimes attributed to it, depends on the irritation which it quickly causes in the part. In this opinion we altogether coincide. The arsenious acid of the shops is soluble in boiling water in the proportion of 115 to 1000 parts, and 29 parts are retained on cooling;

* In compliance with the suggestion of a subscriber, we shall give a paper on the mode of detecting nitric acid, iodine, and the hydriodate of potash, in a subsequent number, in order to render the series of observations complete; although these articles have already been noticed by a correspondent at considerable length.

temperate water again takes up, in 36 hours, 12·5.† The solubility of the acid in water is impaired considerably by the presence of various organic materials, such as mucus, albumen, or astringent matter.

The arsenious acid forms salts with the various salifiable bases, of which the most remarkable are the arsenites of silver, copper, lead, lime, potash, and ammonia, all of which may be prepared either by bringing the arsenic acid into direct contact with the base, or by decomposing a salt of the base (such as the muriate of lime, nitrate of silver, acetate of lead, or sulphate of copper), by means of a soluble neutral arsenite. Arsenious acid added by itself to one of these salts, produces no decomposition, since its affinity for the base is weaker than that of the acid with which the base was previously associated. This fact is of the utmost importance, and deserves to be attentively studied.

The arsenite of copper is a green compound, formed by adding the arsenite of potash, soda, or ammonia, to the sulphate of copper. The arsenite of silver is yellow, and formed with the nitrate of silver in the same way. The arsenite of lead and lime are both white.

The arsenic acid never comes under the notice of the toxicologist in its free state, but it frequently occurs in combination with potash, as the arsenite of that alkali. This compound is formed by deflagrating arsenious acid with nitrate of potash, by which it obtains another atom of oxygen. Arsenic acid is produced, which unites with part of the potash, forming a neutral salt; the nitrate of silver added to the salt (both in solution), causes the precipitate of a brown-red arseniate of silver.

Of the sulphurets of arsenic, two only are of toxicological importance, namely, the pure orpiment and the impure king's yellow, the former occurs abundantly as a natural product, and is artificially produced when sulphur is treated with arsenious acid, or when sulphuretted hydrogen is passed through a solution of that substance. Both these sulphurets of arsenic are exceedingly soluble in alkaline solutions.

Such are the several combinations of medico-legal interest, which the metal arsenic forms, directly and indirectly with other simple substances. The certain detection or recognition of arsenic, whether in substance or solutions, means the procuring of a substance from which the metal may be obtained, and the identity of the metal is afterwards to be proved by the formation of as many of its compounds as the quantity will admit of. The greater the number of these compounds obtained, the more perfect the evidence of arsenic becomes. We shall presently ascertain how the search is to be conducted.

We will now suppose that we are called on to examine the contents of the stomach, or the vomited matter of an individual supposed to have swallowed an arsenical poison. In this admixture, the arsenic may be either in the solid or fluid state. It must also be recollected, that we should be prepared to meet cases in which the fatal preparation was not the arsenious acid or common white arsenic. Instances are already on record, of poisoning by the arsenite of copper, by the arseniate of potass, and by the yellow sulphuret of arsenic. Moreover, when arsenious acid proves fatal, and small quantities of it remain in the stomach and alimentary canal for some time after death, it is liable to be converted into the *sulphuret* of arsenic by sulphuretted hydrogen gas, naturally evolved in these situations. Dr. Christison has described some cases of this kind. To distinguish these in the unmixed pure state, is a matter of comparatively little difficulty, and for this, Dr. Christison has given us amply adequate directions; but the case is altogether different when the poison becomes mingled with the heterogeneous organic materials of the alimentary canal, and though the merit of Dr. Christison's directions for detecting arsenious acid in such a state of admixture, is of the highest character, yet the experimentalist will not find in his work any guide to the detection of the sulphuret of arsenic, of the arsenite of copper, or the arsenite of potassa, any or all of which may have been the cause of death, may remain in the stomach, and escape detection by this omission. However, as the arsenious acid is the poison most likely to demand attention in this country, and as we wish to quote Dr. Christison's valuable remarks upon it, we shall restrict ourselves to it alone, in the first

† Guibourt.

place, and we shall subsequently consider by what means the other preparations may be separated from the various mixtures with which they may be associated.

To proceed, therefore, the vomited matters, or contents of the stomach, may contain arsenious acid, either in the solid or fluid state; if solid, it may be so mechanically suspended, that it may subside from the mixture in an insulated state, and may be separated by simple decantation. To ascertain its nature is then simple; if the quantity be comparatively large, say over a grain, it should be divided into two or three portions, one part should be intimately mixed with charcoal or black flux (prepared by deflagrating nitrate of potash one part, with supertartrate of potash two parts), and reduced to the metallic state in the manner so beautifully and correctly described by Dr. Christison in a subsequent extract; the second part should be boiled in distilled water till dissolved, and a drop or two of the solution placed on three different watch crystals; to the first should be added the nitrate of silver in minute quantity at one side, and a small quantity of ammonia from the other; the crystal should then be agitated till the several fluids meet, when, if the solution be of arsenious acid, a yellow precipitate takes place. The second crystal should be similarly treated with the sulphate of copper and ammonia, when a deep green deposition is obtained; the third should be treated with a drop of water impregnated with sulphuretted hydrogen: an easier way is to collect a little of the gas in a moistened bladder, furnished with a small glass tube and stop-cock,—by compressing this gently with the hand, a stream can be directed against the suspected solution, though it should not be greater in quantity than the 50th part of one drop, and a yellow stain or precipitate will be instantly produced, if arsenious acid be present.

We have now obtained the metal, the sulphuret of arsenic, the arsenite of copper, the arsenite of silver; analysis can scarcely be more complete, and to render it perfect, it is only necessary to deflagrate the remaining portion with about ten times its volume of nitre in a glass tube (by heating the closed end of the tube in the flame of a spirit, till the nitre melts, and continuing the heat till red fumes cease to be disengaged). Arsenic acid is thus produced, giving rise to the formation of the arseniate of potash; on dissolving this in water, and adding the nitrate of silver, a brick-red precipitate is thrown down—the arseniate of silver. The analysis is now logically perfect, since all the compounds of arsenic have been formed. If, however, not more than the tenth of a grain be procured in the solid form, it should be reduced at once, according to the plan thus described by Dr. Christison:—

"The only instrument which should be used by the inexperienced, and the instrument which the chemist will always prefer when it is at hand, is a glass tube. When the quantity of the oxide is very small, it should not exceed an eighth of an inch in diameter.

"The proper material for reducing the oxide of arsenic is freshly-ignited charcoal. With this substance the whole metal of the oxide of arsenic is disengaged. The black flux, which is usually recommended, is ineligible, if the quantity of oxide is very small; for only a part of the metal is disengaged, the remainder continuing in the flux, probably in the form of arseniuret of potassium. If the quantity operated on is large, it should be mixed with the charcoal or flux before it is introduced into the tube; if on the other hand it is small, a better plan is to drop it into the tube and cover it over with charcoal. The materials are to be introduced along a little triangular gutter of stiff paper, if the tube is large; but with a small tube it is preferable to use a little brass funnel, to which a brass or silver wire is previously fitted, for pushing the matter down when it adheres. In either of these ways the side of the tube is kept quite clean, which is a point of great consequence, especially when the black flux is used. In delicate experiments the material should not be closely impacted in the tube. By far the best method of applying heat is with the spirit-lamp, as first suggested by Mr. Phillips. The upper part of the material ought to be heated first, and with a very small flame. Afterwards the heat should be applied to the bottom of the tube, the flame being previously enlarged by drawing out the wick with a pair of forceps. A little water, disengaged in the first instance, should be removed with a roll of filtering paper, before a sufficient heat is applied to sublime the metal. Whenever the dark crust begins to form, the tube should be held quite steady, and in the same part of the flame. By these precautions a well-defined crust will be procured with facility even by a mere tyro in practical chemistry, as I have ascertained by repeated trials.

Dr. Christison has, in this manner, obtained an arsenical crust, which weighed but the 286th part of a grain, and covering a space of the size of the adjoined parallelogram [ ]. Further, happily illustrates the possibility of operating on so minute a quantity by the *mechanical* divisibility of gold, a grain of which can be beat into several thousand visible points. The obtaining of the crust, however, is not sufficient, inasmuch as it has been stated by a high authority, though certainly erroneously, that reduced antimony will undergo sublimation also. Another has also started the objection of the similar appearance sometimes produced by charcoal alone. A third objects to the stain which is produced by heating glass, owing to the deposition of charcoal from the flame on its surfaces. It is undoubtedly true, that these last present a highly metallic appearance; discrimination therefore becomes necessary, and this Dr. Christison (following the judicious suggestion of Dr. Turner) proposes to effect in the following manner:—

"The best method of applying this part of the test, is to heat the ball containing the flux deprived of arsenic, to attach a bit of glass tube to its end, and to draw it gently off in the spirit-flame, taking care to prevent the flux being driven forward on the crust. This being done, the whole crust, or, if it is large, a portion of it, is to be chased up and down the tube with a small spirit-lamp flame, till it is all converted into a white powder. In order to show the crystalline form of the powder distinctly, let the flame be reduced to the volume of a pea by drawing in the wick, and let the part of the tube containing the oxide, be held half an inch or an inch above it. By repeated trials, sparkling crystals will at length be formed, which are octahedres,—the crystalline form of arsenious acid. The triangular facettes of the octahedres may be sometimes seen with the naked eye, though the original crust was only a fiftieth of a grain, or even less; and they may be always seen with a lens of four powers, the tube being held between the eye and a lighted candle, or a ray of sunshine, either of which is preferable to the diffuse daylight for making this observation. For the success of the oxidation test, it is indispensable that the inside of the tube be not soiled with the flux, if the flux contained an alkali; because the alkali would unite with the oxide. It is also requisite not to heat the tube suddenly, so as to redden it before the oxide is sublimed, because then the oxide unites with the glass, forming a white opaque enamel."

It is perfectly easy to go beyond this, even should there be only a single crystal obtained, by introducing with a capillary tube into the original tube a few drops of water impregnated weakly with ammonia. When the crystalline matter is dissolved, the fluid should be dropped on three watch crystals, and the set of experiments performed which have already been described.

Such is the mode of obtaining unimpeachable evidence of the nature of a white powder suspected to be arsenic. In our account we have gone a little further than the eminent author, and introduced some manipulatory directions which are not to be found in his text, we allude especially to the crystal experiments; we have also omitted the description he gives of a peculiar tube recommended by Berzelius to be used in this analysis for the reductions; we object to it, from the conviction afforded us by innumerable trials, that the best form of tube is one at least six inches long, and of the diameter of a middle-sized quill. The length especially facilitates the oxidation experiment, and prevents the necessity for the awkward, difficult, and inelegant mode of drawing out the bulb of the tube Berzelius recommends.

Our space obliges us to pause at this stage of the inquiry, and defer till next week our notice of the more difficult and important analysis of an organic mixture, in which the arsenic has either been dissolved, or so intimately mixed in the solid state, that it is no longer separable by subsidence and decantation.

Before concluding, we avail ourselves of this occasion to describe a mode by which glass-tubes may be prepared by the rural experimentalist in any situation—not so regular in diameter, or elegant in shape, it is true, as the glass-houses can furnish, but of equal utility in the prosecution of analysis.

A thin, white glass ounce phial should be selected for the purpose, and its collar bound with a circle of copper wire, to which should be attached a second piece of wire about three feet long in the line of the perpendicular axis of the phial. The other end of the bottle should also be firmly encompassed with wire, with a short wire terminating in a hook. A small loop of iron should then be driven into the bed of a smith's forge, the

prepared phial hooked on, and the whole urged to redness by a gently increased heat; the phial yields, and may be drawn into tubes of any diameter, by the operator taking the long wire, and steadily retiring from the fire. When the tube cools, it may be cut into pieces by a file, and any of these pieces may be hermetically sealed in a spirit-lamp flame. It is true that six phials may be lost or cracked in the operation, and that an hour or two may also be spent in futile attempts, but one will certainly succeed, and it then affords the experimentalist an abundant supply for his future researches.

Deadly Adulteration and Slow Poisoning; or, Disease and Death in the Pot and the Bottle; in which the Blood-empoisoning and Life-destroying Adulterations of Wines, Spirits, Beer, Bread, Flour, Tea, Sugar, Spices, Cheesemongery, Pastry, Confectionary, Medicines, &c., are laid open to the Public, with Tests or Methods for ascertaining and detecting the Fraudulent and Deleterious Adulterations, and the good and bad qualities of those Articles: with an Exposé of Medical Empiricism and Imposture, Quacks and Quackery, Regular and Irregular, Legitimate and Illegitimate; and the Frauds and Mal-practices of Pawnbrokers and Madhouse keepers. By an Enemy of Fraud and Villany. London: Sherwood and Co. 12mo. pp. 187. 1830.

We are too well aware of the great facility with which epidemic terror is excited by tales of the adulterations in food and drink, to lend our pages to the aggravation of the evil upon the outcry of every visionary,—perhaps knavish declaimer. At the same time, we consider it a duty never to permit a proved fraud of this pernicious description to escape unnoticed. We are equally enemies to needless alarm, and to the too generous confidence which is sometimes reposed on the caterers of the necessaries of existence. It would be difficult, we believe, to determine which of these causes operates with the more injurious influence, and it is under this conviction that we proceed to bestow a few remarks on the publication of the above oddly designated work.

This strange, but interesting book, is evidently the production of a man of considerable talents, though whimsical mind, and superficial in information on some important particulars. He has followed in the steps of the celebrated Accum to a certain extent, and this notorious author he certainly equals, if he does not excel him, in the industry and sagacity with which he penetrates into the arcana of various trades and mysteries, the deceptions of which, whether actual or pretended, he proclaims to the country in no very complimentary terms. His list of adulterations, as may be seen from the title, forms a lengthened catalogue, and extends almost to every item in our daily consumption. Our bread, our tea and sugar, our fruit, wine, cheese, spirits, porter, &c., nay, even our meats, have their several imitations; nothing that we eat or drink, according to the author, has escaped the infernal traffickers. We quote, as an example, his enumeration of the various alien ingredients with which he asserts that gin is occasionally associated:—

"The ripe taste which rum or brandy that has been long kept in oaken casks obtains, is imparted to new brandy and rum by means of a spirituous tincture of raisin-stones and oak saw-dust. And the water distilled from cherry-laurel-leaves is frequently mixed with brandy and other spirituous liquors to impart to them the flavour of the cordial called noyeau. Sugar of lead not unfrequently forms part of the flavouring ingredients of the retailers' rums. But the perfection of adulteration is in gin,—cheap gin—"the *real* comfort,"—patronized by the poor for its supposed GENUINENESS! This infernal compound of combustibles is distinguished from the other slow poisons to which a large portion of the population of 'the queen of cities,'—our 'modern Carthage,' make themselves the willing victims, by the poisonous nature of the ingredients of which it is composed. These are the oils of vitriol, turpentine, juniper, cassia, caraways, and almonds, sulphuric ether or phosphorus, extracts of orris-root, angelica-root, capsicums or grains of paradise, sugar, and heading. The aid of lime-water and of spirits of wine is also invoked in the course of the operation. The purposes of these mischievous ingredients are as follow:—the oil of vitriol is to impart pungency and the appearance of strength, when the liquor is applied to the nose, while the extract of capsicums or of grains of paradise is designed to perform the same office for the taste. The extracts of orris and angelica roots give a fulness of body,

and the coveted flavour called cordial, to the large proportion of the compound, which consists only of water. The remaining oils are to give strength, the sugar to sweeten the composition, and the lime to unite the oils with the spirit; while the sulphuric ether, phosphorus, and heading, are intended to give the semblance of being highly spirituous, from the fiery taste, and the appearance of the light bead which is caused to appear and remain for some time on the surface of the noxious compound. The introduction of the white arsenic is intended to promote an irritable and feverish thirst, so that the poor deluded consumer may be compelled to have recourse to fresh potations of the 'liquid fire.'"

There are many benefits and many corresponding evils, which may arise from such statements as those we have extracted, according to the deliberation or inconsiderateness with which they are put forth. If the fearful and disgusting list be constructed upon actual observation,—if chemical evidence, based on analysis, be added,—if individual malefactors be selected and held up to public animadversion, then the author effects a noble and a patriotic purpose; but if, on the contrary, impeachments are issued against craft and trades, unsupported by chemical facts or particular and well-authenticated statements,—or, as the case may be, if these accusations are made for the sinister object of extracting a pecuniary income from the tribute of general terror and unnecessary excitement, then the evil becomes paramount indeed; for, while needless apprehensions usurp the place of judgment and good sense, fear and disgust convert our most wholesome aliments into poisons more pernicious than the adulterations pretended to be decried.

On one point the author has certainly erred, he mixes the innocent with the guilty in several of his lists of adulterations, and, unmindful of the general ignorance which prevails on scientific nomenclature, he heedlessly or ignorantly associates the names of ingredients, some of the most harmless—some of the most pernicious kind. The preceding extract exemplifies this particular fault, and we subjoin another instance of the same description:—

"The 'curious old soft-flavoured Cognac, ten years old,' of those nefarious dealers, is compounded of Spanish or Bourdeaux brandy, neutral-flavoured rum, rectified spirits, British brandy, British brandy bitters, cherry-laurel-water, extract of almond cake, extract of capsicums, or of grains of paradise, burnt sugar, or colouring matter. But more generally, that '*medicinal*' compound, British brandy, is palmed on the public for real Cognac brandy. This diabolical farrago of mischievous ingredients, which was held forth to the public by interested individuals concerned in the undertaking, as calculated 'entirely to supersede the use of Cognac brandy,' and 'likely to prove of great benefit to the *health* and *comfort* of the poorer and middling classes of society,' is compounded of oil of vitriol, vinegar, nitrum dulce, tincture of raisin stones, tinctura japonica, cherry-laurel-water, extracts of capsicums or of grains of paradise, orris-root, cassia-buds, bitter almond meal, colouring matter, &c., from which enumeration of '*neat*' articles it appears that this 'almost superior brandy to Cognac,' as its modest manufacturers term it, is a slow poison, and equally deleterious in its effects, if not more so, than that vile composition—'cheap gin.'"

One of the most important points to be determined in the consideration of such a treatise as the present, is of course, the veracity of the author; of this, the chief evidence of the affirmative in the case now before us is, in the first place, the want of any evil motive which could induce him to come forward; for, setting a love of mischief out of the question, it may be well supposed that the suppression of such disclosures might be a much more profitable traffic than the sale of the little work in which they are announced. Secondly, he writes in a tone of half-mad honesty, which it is difficult to disbelieve. On the other hand, the principal indications of thoughtlessness (not to say worse), consist in the absence of names and dates and places from his original statements, in the declamatory and puffing style into which he continually lapses, and in the want of satisfactory chemical evidence on some of the most important particulars. Thus he speaks of the adulteration of green Stilton with *verdigris*, yet conceals the source of his information, and adduces no experimental evidence (which might have been obtained with so much facility) on the statement he thus circulates, and apparently on such questionable grounds.

Another circumstance, too, which should in some degree diminish our confidence in this writer's authority, is the inaccurate chemical statements he continually thrusts

forward, and the utter physiological ignorance he so frequently betrays; thus, at page 81, we find him giving credence to the ridiculous story of calves being fed on milk and *chalk*, in order to *whiten* their flesh. We have not space to select more examples of this kind, but they are so abundant, that they meet the eye in almost every second page.

Under all these circumstances, it is not easy to decide on the light in which this publication should be regarded; our own opinion, however, we have no hesitation in declaring to be, that the author is a correct well-meaning individual, but of that class of exaggerating alarmists, which magnifies terrors of this description to a most nonsensical extent. One service he has at any rate rendered to the public, and to this point we would earnestly solicit the attention of our readers, especially those conversant with analytic researches; he has afforded them, in several examples, a clue to the detection of some infamous deceptions, and has set them, we believe, in the right path for the substantiation of the charges which he vaguely promulgates.

In conclusion, though we have already afforded to the consideration of this subject a greater space than it may apparently deserve, we feel it necessary to press upon the general public, that the word "adulteration" is not necessarily synonymous with injury to health, and that hundreds of these deceptions are practised with the sole view of baffling the intolerable oppression of fiscal exactions. We can fancy the valetudinarian peruser of a treatise like the present gasping in ignorant horror at the story of his porter being "adulterated" with quassia, his cheese tinctured with anatto, or his port-wine roughened by the alcohol infusion of tannin; yet these substitutions, though less delicate to the epicure's taste, are as free from any noxious quality in the proportions in which they are employed, as the most genuine article which can be procured. If writers on this subject separated the noxious from the harmless, and dealt not so much in hyperbolical declamation, there would, at the same time, be less terror created, and the ends of public justice would be more effectually attained.

ST. THOMAS'S HOSPITAL.

CLINICAL LECTURE

DELIVERED BY

DR. ELLIOTSON,

Dec. 20, 1830.

DISEASE OF THE HEART.

I HAVE before me, Gentlemen, a diseased heart, which was not taken from a patient in the hospital, but was presented to me by the gentleman who attended the patient. It is a case of the most extensive excrescences from the valves of this organ I have yet seen. The one I showed you the other day was the most extensive instance of the affection I had then seen, but this surpasses it. I have never seen any representation equal to it in any plate, nor have I seen any-thing like it in any museum. Before I enter into a description of the case, I will show round the organ. This is the left ventricle laid open; these are the three aortic valves; and here is the mitral valve. The valves of the aorta and the mitral valve are, you know, continuous, and the excrescences are upon both; they are upon that portion of the outside of the mitral valve which is nearest to the semi-lunar valves of the aorta—we might say, upon part of the root of the mitral valve. One of the excrescences upon this part is of a most extraordinary length—so long as to reach considerably more than half way towards the apex of the ventricle. You see some of them upon all the aortic valves, but the great mass of them is upon the external part of the ring of the mitral valve, nearest to the valves of the aorta.

Through the kindness of the gentleman who gave me this specimen, I had two opportunities of listening to the symptoms in the man from whom it was taken; one of them occurred in July, and one a month before his death, which happened about ten days ago. He complained of shortness of breath, and of debility, and he himself discovered that he had a peculiar sound in his heart before any practitioner had attended him. The sound which his heart made was exactly like that of a pigeon cooing, so that on standing a foot from the patient you might hear it quite distinctly. This sound I examined very accurately, and found that it took place after the pulse. First of all there was the stroke of his heart, and at the same moment his pulse, but sometimes there was a most minute interval between them, so that the stroke of the heart was followed

instantaneously; it was all but accompanied by the pulse at the wrist; sometimes they were observed to be quite simultaneous; and after the pulse was clearly over, then came this cooing sound; after which there was a dead pause; and then the stroke of the heart and the pulse began, followed by the cooing, and this followed by the pause again. Laennec believes that the auricle contracts after the ventricle. I was quite certain that there was an impediment in the man's heart, and I was certain that it was in the left side, because the sound was loudest under the left cartilages of the ribs: it might be heard all over, but it was loudest here, and as you came to the sternum and right cartilages it grew weaker. Then I could not conceive that it arose from any obstruction at the mouth of the aorta, for this reason, that it took place after the heart had given its stroke; or if you choose to suppose that there might be an error in that respect, and object to this statement, I will say that it took place after the pulse at the wrist, after the blood had been emitted from the ventricle. Another circumstance shows that it could not have arisen from the mouth of the aorta being obstructed; the pulse was remarkably full; it was not such a pulse as you have when the blood is so obstructed in its course into the aorta as to give a very loud sound. You cannot form a perfect idea of the state of things from the heart as you at present see it. These excrescences now look flat, but at the time the body was opened they were full and plump: they have now been hardened by spirits, but before that they were all plump, so that at their base they presented a cauliflower sort of appearance, from which a few very long shoots extended. The mass, which was so exceedingly full, solid, and plump, consisted of those excrescences which I now show you, and they altogether formed a large body. There was thus a considerable mass pressing externally upon the mitral valve. You see that these excrescences are seated on the outside of the mitral valve, and there is apparently no obstruction in the valve itself, the opening being as large as natural; but when the heart, which is now laid open, was together and entire, this mass of excrescences must clearly have pressed very considerably down upon the mitral valve, and narrowed the auriculo-ventricular opening. I do not think that the excrescences upon the mouth of the aorta produced any obstruction whatever, for you will observe that the aorta is clearly enlarged; the excrescences must have been to a great extent in the way, but yet the valves are considerably larger than natural; they, and the tube of the aorta, were so dilated that the impediment that would have been afforded was obviated. You do not in any healthy heart see valves of these great dimensions. The mouth of the aorta is larger than it should be, and there is a full opening and a full passage, notwithstanding the excrescences, for the blood to escape from the left ventricle. But here was a great mass of solid matter exactly at the root of the mitral valve, and it appeared certain that these exerted considerable pressure, not on the margin of the valve, but exactly where the blood leaves the left auricle. The excrescences that produced the obstruction were external to the valve, but you know that when there is an obstruction in the mitral valve itself, this becomes indurated, and the opening into which I now pass my finger becomes a mere slit. It is quite certain that there was no obstruction at the mouth of the aorta, because the pulse was full, and the bellows-sound took place clearly after it.

You observe that the left ventricle is larger than it should be; it is dilated, and when the heart was first opened it was much thicker; now it is condensed by the spirit. The signs of hypertrophy were very clear. He had a strong action of the left ventricle—a very great impulse, without any alteration in the usual noise. When there is great thickening, without any dilatation, you have a diminution of sound; but when both occurrences take place in equal proportion, I believe you have much about the natural sound. Here the action was very great, raising the stethoscope and the head, and it was certain that he was labouring under hypertrophy of the left ventricle, with dilatation.

I will now mention a case which proved fatal in the hospital since we last met, for the purpose of comparison with this, instead of introducing it in its regular order. A woman was admitted, on the 2d Dec., into Mary's Ward, æt. 25: she had been ill five months. She laboured under disease of the heart, and consequent dropsy. In her case the symptoms were universal dropsy, difficulty of breathing, loud action of the ventricles, and a bellows sound at the apex of the heart in the situation of the left ventricle. These things frequently occur in persons as young as these were—the man being under thirty, and the woman under twenty-five; and they also generally occur in consequence of pericarditis, and pericarditis as the result or attendant of rheumatism. The man whose heart I have now shown you had laboured under rheumatism, and that he had had pericarditis was indicated by a circumstance which I can now show you—the deposition of thick fibrine upon the pericardium. This was strong presumption of former pericarditis. The woman had had rheumatism, but not to an intense degree. It was only on ques-

tioning her very minutely that I could discover that she had had rheumatism; but she said that she had had pains all over her—that she had had pains in her back and shoulders, and that her limbs had become stiff, hot, and swelled. She had clearly had acute rheumatism, but did not call it so; and I should have passed it over, had I not been aware that few young persons have disease of the heart without previous rheumatism, and indeed without previous pericarditis, and questioned her very minutely. After all these rheumatic symptoms had occurred, she began to swell first in the ancles, then higher up; her breath became short, and when she was admitted to the hospital she had general dropsy, a degree of feebleness and flabbiness of the whole body, and the heart beat to a greater extent than usual. There was a louder noise of the ventricles than usual, particularly I thought of the left, and at the apex there was a bellows sound. She had, of course, difficulty of breathing, and on striking over the region of the heart there was a dull sound to a very great extent. I considered this woman as labouring under pericarditis, in some degree, as well as organic disease; for there was tenderness over the region of the heart. I found the chest universally tender on pressure, but particularly so over the region of the heart. Leeches were applied and colchicum given, and she was put on low diet. She became very cross and dissatisfied at having low diet, and said nothing of that weak kind would do for her, and she ought to have meat, and wine or porter. I told her that it was not proper for her, but this made her so cross that she fell into a violent passion after I left the ward, and I was informed that she agitated herself to such an excess that pain in the region of the heart came on, and Mr. Whitford, jun. applied sixteen leeches in consequence. In a few hours after this pain had begun she suddenly expired, and I have no doubt that the woman died from mental emotion. When a person is labouring under disease of the heart, nothing is more dangerous for them than to suffer considerable emotion of any kind.

On opening the body both ventricles were found dilated; the left so much so, that the apex had become excessively thin, and no doubt, if she had lived much longer, the heart would have burst at that spot. The bellows sound in her case did not occur as it did in the man; it took place at the moment of the pulse. I see, in the note which I made on her admission, that I have entered "loud action at the left ventricle, with a bellows sound at the apex." The valves of the aorta were found considerably diseased. The specimen, I thought, was put away carefully, but I am afraid it is now lost; the man who was entrusted with it has lost it. It was a beautiful specimen, though much inferior to that which I have exhibited; I should have felt great pleasure in comparing them together, because the bellows sound was heard at two distinct times: in the man it took place immediately after the pulse, and presented a cooing sound; in the woman it occurred at the time of the pulse, and was a blowing sound. In the woman no disease of other organs was found. The mouth of the aorta was not dilated in her case, so as to make up for the obstruction which arose from the excrescences of the valves. No excrescences grew upon the mitral valve, except a small portion at the margin, which, however, clearly could not have in the least interfered with the function. A large quantity of clear yellowish serum existed in the pericardium.

I now proceed, Gentlemen, to take a hasty review of the cases which have been presented, but have not yet been spoken of in my lectures.

CASES OF IMPETIGO AND ECZEMA.—DISEASES OF THE SKIN GENERALLY.

Among the women two cases were presented of impetigo, and they were exceedingly interesting, not as being illustrative of the disease, because that is so common that no one can be long without an opportunity of seeing it; but from the success of the treatment which was adopted, from the illustration it affords of the success which generally follows a particular mode of treating the disease. You are aware that impetigo is a pustular disease, that it is not contagious, and that it is characterized by those pustules which are called *psydracia*; that is to say, by small pustules very little raised. In porrigo the pustules are acuminated, and some of them being pointed, are called *achores*; or else they are large, and called *favi*, the scabs which are formed being full of little cells like the honey-comb. But in impetigo the pustules are *psydracia*,—small pustules, not pointed, and very little raised. The disease for the most part becomes chronic; and unless you adopt one particular sort of treatment, it is often a very obstinate affection. The girl of whom I am now about to speak, was admitted with impetigo of her arms and various other parts of her body. *I found that she had headach; that she was heavy, drowsy, sometimes giddy, and that her pulse was full; she was therefore bled in the arm, and the blood was buffed and cupped.* The treatment consisted in putting her on low diet, bleeding her from time to time (the blood was always buffy, and sometimes cupped), and giving her mercury moderately. By perseverance in this treatment, and nothing more than this, the girl

got so well that it was not worth while to keep her in the hospital any longer, and she was presented on the 2nd of December.

The case of a woman in the same ward was similar, only that the pustules were not so fully defined; their contents were in some measure watery, and you therefore had just as much reason to call it eczema as impetigo. You know that in eczema there are not pustules, but vesicles, with a considerable degree of inflammation around them, just as in impetigo; but the two diseases run into each other. There is one form of eczema so much like impetigo, that Dr. Willan, in his invaluable work (we possess others more recent, which are also excellent), calls it *eczema impetiginodes*; but they are the same affection, with this only difference, that sometimes the contents are serous, and sometimes purulent. The treatment, so far as I have observed, is exactly the same. This woman was put on low diet like the girl; she had been ill four months before her admission, without any improvement taking place; she was bled, and the blood was buffed and cupped; she was put on low diet, and mercury was given in small quantities, so as slightly to affect her mouth. She came in on the 11th November, and was presented all but well (so well that it would have been an absurdity to have kept her in the house) on the 2nd December. *In her case there were, heat of body, headach, giddiness, and thirst.*

Now in many books written on diseases of the skin, there is this great defect, that they do not point out that certain internal parts, or the constitution, in this disease, is so frequently in an inflammatory state as it is. The French seem to be aware of the frequency of internal affection; but they say that it is always the stomach and intestines that are inflamed, that there is a gastro-enteritis, and that this gives rise to the heat, nausea, oppression, and so forth. However, I am satisfied of the necessity of considering cutaneous diseases in a more pathological point than they frequently are regarded. It is highly proper to know their external characters very minutely, for that enables us to recognise and distinguish them, and to communicate our ideas of them to each other; but if we go no farther than the variety of their external characters—if we merely speak of them as *naturalists* (dare I use the term?), we shall form but a very indifferent conception of the diseases; it is absolutely necessary to take a *pathological* view of them, and consider the whole state of the system. You will continually find that the affection is much more than *skin deep*; you will continually find, even when cutaneous affections have long been chronic, some signs of an inflammatory state of the system, particularly of the head. As far as I have been able to discover, the head much more frequently suffers than the abdomen. These two patients had no tenderness of the epigastrium, and in a very great number of cases you will not find signs of inflammation there; but you very frequently find the head disturbed; and even in those cases where the stomach and intestines are inflamed, you will find that the head also frequently suffers. I can only speak from what I observe; and certainly, in my practice in London, it is the head that suffers in these diseases, from an inflammatory condition internally, much more than any other part. However this may be, you will be surprised to see the blood buffed and cupped so frequently as it is. Often in lepra, psoriasis, impetigo, &c. of many months', and even of many years' duration, do I find the blood buffed and cupped. You will find, whatever treatment you adopt, and whatever good it may do, it will often ultimately fail in effecting a cure, unless you put the patient under anti-inflammatory treatment. I believe you will fail in this disease, as in epilepsy, with many excellent drugs, through the inflammatory state of the system not being sufficiently attended to. Farther, though you both bleed and purge, you will do little good if you allow the patient a diet, the direct tendency of which is to counteract the effect of those measures. It is necessary to put the patient on moderate diet, to take away some of his stimuli,—his wine, beer, spirits, and sometimes his meat,—and perhaps to bleed him from time to time. The state of the skin will frequently not indicate bleeding, when the headach, drowsiness, and vertigo, do; and thus a course of antiphlogistic treatment is often demanded in cutaneous diseases. The French are quite aware of the necessity of bleeding in cutaneous diseases, but they bleed locally, by the application of leeches around the affected spots, or to the epigastrium; and though doubtless they effect great good, yet you will find that bleeding at the arm is frequently necessary on account of the state of the head, and that it answers every purpose. In many chronic diseases of the skin, where even no inflammatory signs appear in the head or abdomen, venesection at once lessens the redness, heat, and itching, and will soon cure the disease. In some of the best books, even the inflammatory state of the skin itself is scarcely attended to; or if its existence is implied, it is not dwelt upon as important in many chronic cutaneous diseases; when also internal affection is mentioned, it is too frequently described as ceasing on the appearance of the eruption, or its true character is not minutely given, and post-mortem appearances are not described, even with respect to measles, scarlet fever, or small-pox. Even so common and

innocent a disease as urticaria is often imperfectly described; the inflammation and swelling of the tongue and fauces, such occasionally as cause great distress, are passed over, and, necessarily, the frequent cupped and buffy state of the blood, since venesection is not attended to, although, even when the disease has arisen from something that has been swallowed, it presently causes the acute form to recede, frequently the same day,—not unfrequently indeed while the blood is flowing, and the patient is thus saved many days of misery.

All diseases of the skin, however, are not inflammatory, neither do they all require antiphlogistic treatment. They resemble dropsies, discharges from mucous membranes, hæmorrhages, and most other diseases; some are inflammatory, and are to be cured only by bleeding, purging, and low diet: some are attended by no inflammation, but by debility, and require stimulants and tonics: some demand a middle course, consisting of moderate antiphlogistics, with tonics and stimulants; some, though inflammatory, have something more than inflammation in them, and cannot be cured by bleeding, while others are greatly influenced by specific or other peculiar drugs, and will yield to those only, though they will do so sometimes the more readily if antiphlogistics are also had recourse to. In this particular form of disease, impetigo, or eczema, which bears so strong a resemblance to it, when so far of an impetiginous character, that you may call it *eczematic impetigo*, you will find mercury of great use. In many cutaneous diseases mercury is of no service, but that is not the case in impetigo. When I have backed mercury by moderate bleeding, and by moderate diet, I have generally found it answer excellently well in this disease.

With respect to local applications, it is necessary to remember that stimulants are for the most part injurious. There is irritation enough of the skin already, and that you must soothe. Frequently I have tried cold soft water, not pump or hard water, with the most beneficial results, and this you will find one of the best applications that can be employed. If you apply anything else, I think starch, or the oxyde of zinc, answers very well. If the patient's skin be not irritated by grease, unguentum zinci is one of the best ointments you can resort to. Dilute solutions of the chlorides are sometimes beneficial. Although strict antiphlogistic remedies are so often indispensable in this and other cutaneous diseases, an opposite state of the system is continually seen; that is, a state of the system in which tonics and good living are demanded.

CASE OF AMENORRHŒA.—ANÆMIA.

There was presented during the week one other woman, and she laboured under amenorrhœa. It seemed to be an amenorrhœa arising from a deficiency of blood and of activity, and on that account I gave her steel, and she very soon got well enough to go out.

You are aware that the treatment of amenorrhœa must be very different in different circumstances. Sometimes it will occur from a fulness of the whole system and of the uterus; but in other cases it will occur from an opposite cause;—it will arise when the whole system is in a state of debility, when there is too little blood, and what there is, is of a watery character. In some cases it is best to open a vein, or to apply cupping-glasses to the loins, while in others the treatment must consist in furnishing the patient with strength and blood wherewith to menstruate. This woman was aged 22, and had been ill six months; she was admitted on the 18th of November. She was pale and excessively weak, and the case was one indicating the propriety of strengthening measures. She took two drachms of subcarbonate of iron three times a day, and went out on the 2nd of December. I met her in the street a day or two ago, looking perfectly well. A state analogous to this often occurs when the spleen is enlarged and indurated. Sometimes there is a disease which is peculiarly called *anæmia*, cases of which have been described by Dr. Combe, in the "Edinburgh Medico-Chirurgical Transactions," and by the French. In all these, the blood has been observed to be deficient both in tenacity and redness; hence the waxen hue of the surface; and it is attended by debility, faintness, a very excitable pulse, and respiration; in short, by all the effects observed after hæmorrhage, or after excessive loss of blood, for it comes to the same thing, whether the blood which is made is lost, and supplied by an excess of serum, or whether blood is generated of too serous a character. In France, the cases of anæmia occurred in persons who worked in a particular gallery of the coal-mines near Valenciennes. The affection, however, which is much the more frequent, is that of young females, called *chlorosis*. But chlorosis may occur in males. I do not mean to say that males experience a want of menstruation, but a state of the whole system, just the same as that of females in chlorosis, happens sometimes in males. In all these cases it has been shown that iron is the best thing that can be employed. In the French cases, the whole of their treatment was unsuccessful till they exhibited iron, and these occurred twenty years ago. Mercury did serious injury. With respect to the anæmia with enlarged spleen, you will find great advantage from iron; this is particularly mention-

ed by Professor Tomassini, in his Clinical Reports; in chlorosis it is the best medicine that can be given; for the chronic state, following excessive loss of blood, it is one of the best remedies; these are all of them forms of anæmia, and in the chlorosis of this female it answered admirably.

GOUT.

Among the men presented was a case of gout; the occurrence of which, in the hospital, is a great rarity. While Sir Gilbert Blane was physician to this hospital, a period of ten years, he never had a case of gout, while, in private practice, he had nearly 150. This shows the great influence of our habits upon this disease. The poor in this country never drink wine, but consume malt liquor and spirits; whereas the rich drink very little of the two latter, while they consume a great deal of the former. The poor drink beer and spirits enough to produce the gout, if those liquors had the property of causing it; and many of them follow such sedentary occupations that we cannot say the power of these fluids is always counteracted by hard work. This is only the third case that I have seen here, during the eight years that I have been physician to the in-patients of the hospital. When it has occurred in patients in St. Thomas's, I believe it has been referable either to poor people having been in particular situations, where they were able to procure wine, or from their having a strong natural, and often hereditary tendency to gout. Many persons in the higher classes live most abstemiously, but they nevertheless have the gout. When the affection has hereditarily got into the system, it will take two or three generations before it can be eradicated. I have seen thin and abstemious persons labouring under gout, and they have been under the necessity of taking wine in moderate quantities, on account of the weakness of their constitution. This man laboured under acute gout of the hands, which were greatly swollen, red, hot, and shining; it originally began in the great toe, in the middle of the night; he had had many fits, and they had long begun at night. He would go to bed well, and awake with violent pain, the parts being red, hot, and shining. He had been subject to the disease for about five years, and suffers two or three attacks annually; he was sixty-six years of age. By the treatment adopted he soon got the better of the complaint. He was treated by vinum colchici, exactly as we give it in acute rheumatism, and as soon as it began to purge him he got well; he took half a drachm three times a-day. He was admitted on the 18th of November, and presented on the 2d of December. In two days the medicine began to purge him, and then he took it but twice a-day, and went on well.

There was a man admitted on the 18th November, and presented on the 2d December, who laboured under gastritis and bronchitis. The symptoms were, great tenderness of the stomach, with heat there and up the throat; sickness, and at the same time short respiration, with copious expectoration and sonorous rattle; all over the chest a sonorous rattle was heard. He got well in the usual way, simply by bleeding, starving, and a few doses of calomel.

DIARRHŒA.

In Jacob's Ward four cases were presented: one a case of moderate pleuritis, which was easily cured, and one of continued fever, which was as easily got the better of. The other two cases were of some little interest. The one was a case merely of diarrhœa, but when the man came into the house I examined his chest very carefully, or I might have supposed that he had disease of the lungs. On listening, his lungs proved to be sound. He said, however, that he had violent diarrhœa, and a cough, and that his legs were swollen. It is common for patients to apply at the hospital with phthisis who never speak of expectoration or cough, but only of purging. This is sometimes done from artifice, as they know that we do not admit patients with phthisis, because we can do them no good. Others, however, suffer so much from the diarrhœa,—it causes them so much trouble,—that they forget the trouble of coughing and difficulty of breathing; so that when patients come with swollen legs and purging, we are always very suspicious that they have phthisis. This man's pulse was quick, and so far he had some signs of phthisis, heat, diarrhœa, swelling of the legs; and he said he had cough, and some little expectoration. On examining the chest, however, the sound proved to be perfectly healthy throughout, and I therefore concluded that by curing the diarrhœa, I should cure him altogether, and that his cough and expectoration were insignificant, and only mentioned from my questioning him closely as to their existence; this was effected simply by giving him opium and good nourishment. The cough and expectoration I could never observe, and, as he gained strength, his legs ceased to swell.

LUMBAGO.

I admitted, at the same time, a case of lumbago of great violence, which was presently cured by what you will often find very appropriate treatment. The man was brought to the hospitable unable either to sit or stand; he was supported by two persons, when I first saw him, on the edge of a bench, and he was drawn so much back

from the violence of the pain, that, at first sight, he appeared to be labouring under opisthotonos. His face was expressive of the most violent agony; his features were contracted, and he roared out with pain. He was drawn back so much that I thought it right to ascertain instantly whether there was any tetanic affection. I found there was no affection of the lower jaw, a symptom which is usually present when tetanus exists in any part of the body; I looked at his hands and his feet, and I found no injury there. Upon further inquiry, I found that he sweated profusely. To ascertain whether the case was one of nephritis, I asked if he had pain in the course of the ureters, and down the inside of the thighs; if he had retraction of the testicle, or if he had a frequent desire to make water; and if the pain was confined to one side of his body, and if he vomited. To all these things he answered in the negative; there was no reason to suppose that he had nephritis; the case negatively, therefore, appeared to be one of acute lumbago, acute rheumatism of the loins; and there was tenderness over all the loins, and profuse sweating, exactly as in acute rheumatism of any part. In acute lumbago, the heat of the body is sometimes very great, and the pulse very quick: I have seen the one at 108 and the other at 160 degrees. He was instantly cupped over the part to a pint; he had three grains of opium, and then half a drachm of vinum colchici, and the latter was repeated every eight hours; he was instantly relieved, on the second day, was able to sit up, and on the 2nd December he was presented perfectly well, having been well six days. I thought it right, however, to keep him in the hospital, lest there should be a return of the complaint from his catching cold. This is a description of a case to which you may frequently be called; and if only trifling measures are resorted to, it may run on for a considerable time; but you produce great comfort to the patient, and may gain considerable credit to yourself, by using active measures. If I had only taken six or eight ounces of blood from his back, or had given him merely a few grains of Dover's powder, it would have done him no good; but, from his general strength and his age, it made no difference whether he lost a pint of blood or not. He was shortly after able to walk about, and was very grateful for the good done to him. The treatment was simple, but from making a correct diagnosis, and then putting the simple means which were required into full force, it was successful.

EPILEPSY.

If I had had the pleasure of meeting you last Monday, I should have had to report the admission of six cases during the preceding week. Among the women was a case of dilatation of the heart, and disease of the aortic valves (of which I have spoken), and a case of epilepsy. Among the men was another case of epilepsy, a very curious case, in which the fits were preceded by a peculiar sensation, commencing in the foot, running up the leg, and reaching the epigastrium, and then followed by a fit; there was a case of chronic bronchitis, and chronic inflammation of the windpipe and larynx, and a very curious case of spasmodic cough.

With respect to the case of epilepsy, you know that epilepsy is sometimes preceded by a peculiar sensation, generally as if an insect were crawling along the skin. You cannot trace it in the course of any particular nerve; it seems rather to be a sensation in the skin. Sometimes it has its origin from some cause operating in the part where it arises, but in other cases the cause appears to be in the head. There is an instance mentioned by Dr. Curry, at Guy's Hospital, of this aura, as it is called, rising from the extremities, where, after death, a little tumour was found in the head. Now in this man the circumstances are similar. He had pitched on the front of the head, and in consequence there had been first the usual symptoms of concussion, and after that violent pains of the head, which lasted for some time. To these was added epilepsy, and the fits have been always preceded by this peculiar sensation of trickling or creeping about the root of the great-toe. It then runs along the inner part of the foot, behind the inner ancle, along the leg inside the knee, then inside the thigh, and as soon as it gets to the epigastrium, he falls down; the case is attended with violent pain of the head, and violent vomiting. We must suppose that a chronic organic disease is set up; at any rate there is an inflammatory state of the head. The man is much better, though I doubt whether he will be cured even by rigid antiphlogistic means.

SPASM OF THE HAND AND FOOT.

During the week there were six cases disposed of; four cases went out of the hospital, and two patients died. One patient who went out of the hospital was a woman, to whose case I directed your attention at the time of admission; her disease consisted of spasm of one hand and one foot. I mentioned that the woman was brought in with her hand and foot completely turned in, and stated that this was an occurrence which we sometimes see during and after continued fever; I also stated that I found she had extreme tenderness at the nape of the neck, with pain there and at the occiput. The disease I considered to be, in a great

measure, irritation of the nerves which supply the muscles of extension of the right extremities. I told you that I should probably cure her, as the disease was recent, by antiphlogistic measures directed to the back of the head: this was the case. She was well bled at the back of the head and neck; I ordered her to be leeched every day; a large number of leeches to be applied there, after one cupping to the amount of a pint; the first bleeding caused her hand to become straight; two grains of calomel were given twice a day till her mouth became affected, and twenty leeches were daily applied to the occiput from the 11th to the 23d November: by that time she was much improved; some domestic calamity, however, happened at home, and the poor woman was obliged to go. She felt very grateful for the benefit she had received. She had suffered violent pain from tension, as the foot had been bent in so much. The pain was gone off; there was a diminution of the contraction, and she was really approaching to a cure very rapidly; and I dare say that by this time she would have been well, but for the unfortunate circumstance of her being obliged to go home, and look after some of her family.

ITCH.

There was a case of pustular itch presented, of which I spoke at the time of the patient's admission. You are aware that the itch often puts on a form which nearly disguises it; it is accompanied by largo pustules, which are called *phlyzacia*,—large pustules, with a hard base, more or less inflamed, and well distended with pus. Now the itch is sometimes of this highly inflammatory nature, so that what would be vesicles, from the violence of the inflammation becomes pustules; but you will generally, in these cases, ascertain the true nature of the disease by observing that these pustules beset most particularly the hands and wrists, the feet and ancles: and then, in the most usual situations of itch, the roots of the thumbs and great toes, between the fingers, the wrist, and in the axilla, you find minute vesicles,—little elevations with watery heads, some with a black speck on their summit, and all the vesicles and pustules are attended by violent itching—not by the tingling of nettle rash, but by violent itching. It is necessary to observe, that in all cutaneous diseases, if you carefully look all over the body, you will somewhere find the disease in its true and real character. In this man there were these large pustules, but between them there were small vesicles with watery heads; some of them had lost their heads from the man's scratching, and had become black points: the itching was extreme. There was some upon his breast, but none upon his face. I employed as a local application sulphur ointment, and he was soon cured. But after the essence of the disease had been got the better of, four or five pustules were left, and it appeared clear to me that these continued from the friction that was being employed, and that they would not get well if the mechanical irritation were continued. By employing a simple dressing of the same ointment, and allowing no friction, they speedily healed. He had a little ulceration of the leg, but it was not of a specific character, having been excited by his stocking, and it soon got well. There is one peculiar circumstance in itch; I never saw it attack the face. When a person is labouring under itch, he may have an eruption of the face just the same as other people; you may see a little acne on the face, or ten thousand things may be observed there, but the true itch very rarely affects the face, for I never saw an instance of it there.

PALPITATION OF THE HEART.

There were two other cases presented, one of which was rheumatism with coldness, and for which stimulating remedies were employed; but the man got clothed by the Lord Mayor, and he speedily left the hospital. The other was a case of nervous palpitation of the heart. You will continually be consulted by persons for mere nervous palpitation—functional disturbance of the organ. In this case the palpitation arose from nervousness. You will find the heart beating more quickly than natural, and with a louder sound, but you will not observe a greater action at one part than another, nor will the sound appear loudest in one particular situation. The whole of the heart appears to be in a state of morbid irritability, and the consequence is, that every part acts more energetically than it should. In organic disease of the heart there is perhaps one part acting more than the other—one ventricle, one auricle; or if the disease be excessive, you may have both ventricles, or an auricle and a ventricle, or an auricle and both ventricles, in a state of morbid action, acting with morbid force or sound; but it is very rare to meet with this circumstance occurring at every part of the heart. In organic disease, if you have hypertrophy, the sound is lessened though the force is increased; and if the part be dilated, you find not only a local increase of sound, but frequently a dead sound on percussion, showing that there is too much solidity in the chest. Frequently a preternatural sound is heard in the heart, from obstruction to the blood; but in this patient there was no preternatural sound, no deficiency of sound; the action was not more violent at one particular part than at another. It is by these negative circumstances, as well as by the

absence of pulmonary affection and dropsy, that you will ascertain the true nature of the disease. You may be quite right in saying that the case is one of nervous irritation, and yet the person after a time may have organic disease of the heart, because when the heart has been labouring under morbid irritability for a length of time, it is very possible for one part to give way, and to have organic disease set up. This man had pricking pains over the heart, which is a common symptom in nervous palpitation of that organ. There is nothing dangerous in this particular symptom, but the pricking pains are sometimes excessively troublesome. That this has not any-thing to do with organic disease of the heart I am quite certain, because, many years ago, I noticed it over and over again in persons who are now perfectly well. The treatment in the case of this man consisted in the local application of leeches over the region of the heart, in keeping him very quiet, and in keeping his bowels open. Upon this very simple plan he speedily got so much better, that it was not worth while for him to stay any longer in the hospital.

CEREBRAL DISEASE.

During the past week two deaths occurred, the one from phthisis, chronic peripneumony, and chronic pleuritis, in a man; the other in a woman from epilepsy, but whom I never saw. She was admitted one day after my visit, and it was represented that she had had a fit of epilepsy in the street; that she had been insensible, had been convulsed, had foamed at the mouth, and had bitten her tongue. After being put to bed she came to herself, and was sitting up, nothing having been given to her but a dose of aperient medicine, and she said that she then had nothing the matter with her. While, however, she was sitting up in bed, she fell back, I understand, and died. On opening the body, the cristi galli of the ethmoid bone was carious, and the dura mater, where it was attached to this part, had become of great thickness and hardness. The corresponding part of the brain on one side, that is, the inferior part of the anterior lobe on one side, was thoroughly softened. Her history was not known, and I cannot tell whether she had had epileptic fits before; it appeared that there was no paralysis, for she talked and moved her extremities very well, and did not complain of any-thing after the fit was over. The softening of the brain most probably had taken place in consequence of the disease of the dura mater and the ethmoid bone. I presume the part of the brain next the diseased membrane and bone had become diseased first, because when I have seen caries of the temporal bone, which I have more than once in disease of the ear, the brain which was nearest to it likewise became diseased. With respect to the sudden cause of death: The left ventricle of the heart was not found empty as is usual, but filled with blood; it was not contracted at the time of death, and it is probable that the heart suddenly ceased to act. There was no disease whatever found in the heart. I presume the affection in this case must have been sympathetic, that the state of the brain must have operated upon the heart, and caused it suddenly to stop.

OBSTETRIC AUSCULTATION.

Reply of DR. KENNEDY *to* DR. NAGLE.

To the Editor of THE LANCET.

SIR,—I confess I felt somewhat surprised on reading a paper in your publication of the 18th inst., on obstetric auscultation, purporting to be written by a gentleman signing himself David C. E. Nagle, A.M., M.D., T.C.D. How this gentleman could have so grossly (I regret being obliged to use the expression) misrepresented the observations and facts set forth in a paper which I some time since published in the fifth volume of the Dublin Hospital Reports, I am at a loss to conceive. That his doing so could not, however, have arisen from his not understanding me is, I fear, but too evident; as it certainly required more ingenuity to misconstrue, and more art to pervert facts and statements as he has done, than could possibly be necessary to comprehend them. I should therefore deem a lengthened refutation of his paper not only unnecessary, but unbecoming, and shall merely point out a few of his most palpable misstatements, and beg to refer those who may feel interested in the subject, to the paper which I have already published. He asserts (pages 396 and 399) that I, with Dr. Ferguson, assume the "dangerous theory" that the placental sound should be considered an unquestionable test of impregnation; this I deny, these are my expressions:—"The placental soufflet (taken in conjunction with the other symptoms of pregnancy) will tend much to strengthen the opinion of the presence of this state. The co-existence of the fœtal pulsation with the soufflet of course decides the question, but with regard to the latter taken separately, I will go so far as to say, that were all the other symptoms of pregnancy absent, and that this could be distinctly perceived, I should at least withhold my opinion until a sufficient time had elapsed to place the matter beyond a doubt." See Dublin Hos-

pital Reports, vol. v., page 258-9 ; see also pages 255 and 257, where such "dangerous theory" is not only disclaimed but actually condemned. Again he advances, page 398, "That persons, if influenced by my theory, as he styles it, respecting the *quality* of the *soufflet affording a sure indication* of the life or death of a fœtus 'in utero,' would be liable to fall into very serious and egregious errors." That I support any such theory or hold any such opinion as this, I must again in the most unqualified terms deny; that I am fully justified in doing so, will appear from the following, which are my words, when treating of this alteration in the character of the sound, Hospital Reports, page 269:—"The placental sound, either by ceasing altogether after having been previously heard, or having its character altered, from the continuous murmur with its lengthy sibilous termination, to an abrupt, defined, and much shorter sound, *together with the impossibility of detecting the fœtal heart's action, particularly if such has been before observed, places the child's death beyond a doubt.*" Why was the concluding portion of this quotation so cautiously, so unfairly suppressed? It was that he might give a "case" in which, though the fœtus was putrid, he heard a murmur, *prolonged* and by no means "*abrupt*;" evidently insinuating that I deny the possibility of the occurrence of such a case; and he adduces it triumphantly, and "as furnishing him with a powerful argument against the quality of the murmur being a test indicative of the life or death of the fœtus in utero." Here is the "suppressio veri" coupled, as it usually is, with the "assertio falsi," for I have given a case (page 250) where the sound was not "abrupt," although the fœtus was dead, and I also mention having observed such, where the fœtus exhibited marks of having been dead for weeks. With these facts staring him in the face, he asserts, with what justice I leave to you and your readers to determine, that I consider the *quality of the soufflet as affording a sure indication of the life or death of the fœtus in utero.* With regard to his discovery (page 398) that a placenta is not necessary for the production of murmur such as we ordinarily hear in the advanced stages of utero-gestation, and the inference he would draw of my inaccuracy, I beg to refer the reader to a fact, of which, however it may suit Mr. Nagle's views to appear so, *he* can scarcely be ignorant, namely, that I have already stated, without any disguise (page 266), "a case where a sound resembling the soufflet from a morbid cause (a considerably enlarged liver) was observable;" and also mentioned (page 265) that those unacquainted with the stethoscope may be deceived by other sounds, from their resemblance to the placental soufflet, several of which I instanced. So much for misstatements and suppressions with regard to myself. I shall now notice, very briefly, a few of those general statements which are so palpably opposed to fact, that I should deem myself culpable in passing them over without some remark. He says that the "bellows sound (or soufflet) is heard in ninety-nine cases out of every hundred, as well on one side as on the other, in the same patient;" now, although I admit, and have mentioned the fact of its being occasionally heard on both sides in the same patient, I have no hesitation in stating as far as my experience goes, the above proportion is most enormously exaggerated. It (the bellows sound) may, I admit, says he, "be masked in some degree by the pulsations of the fœtal heart." Ridiculous! Can the fœtal pulsation "mask" a sound at least ten times louder than itself? The assertion "that he was never able to trace it (the soufflet) across the anterior surface of the abdominal parietes in an uninterrupted course," may be perfectly true, but if he means that because *he cannot hear it*, it cannot be heard there by others, I set it down as of a piece with those already mentioned, and I doubt whether he is really capable of recognising the phenomenon of which he treats. "Or ever," he adds, "to detect it under the mesial line, except when it arose from the 'epigastric arteries.'" What degree of credit will be attached to the reasonings of a person who displays such ignorance of anatomy? I will venture to say that the merest tyro at the profession could have taught him that in no case do the "epigastric arteries lie under the mesial line;" and that in the abdomen, distended as it is in utero-gestation, each of these arteries lies from four to six inches distant from this line. Again, he speaks of the "sound being traced upwards and forwards towards the mesial line, in the course, as it were, of the trunks of the lateral uterine arteries." The trunk of the lateral uterine artery upwards and forwards and towards the mesial line!! This is indeed "as it were," but not "as it is." If this gentleman had rested satisfied, merely with adopting views and opinions grounded on such data, however incorrect and absurd, and had his positions been supported by inaccuracies and discrepancies more palpably striking than his paper exhibits throughout; it should have remained uncommented on by me, as such productions generally find their own level. I would, however, and with justice, have been to blame as well upon my own account as on that of the profession generally, had I allowed such a tissue of misrepresentation to remain uncontradicted. Having now done so, I must decline taking

any further notice of his observations or entering into any discussion with him on the subject. With regard to Dr. Clinton, whom he endeavours to identify with himself in his views and statements, I confess the only way in which I can reconcile to myself the idea that he ever sanctioned the publication of such a paper, supported by his name (if he really did so), is that he unthinkingly entrusted himself in the hands of the writer of this paper, and acquiesced in his views without taking upon himself to inquire into the subject, and certainly without having read my paper which his name has been brought forward to overthrow, as, unless my opinion of this gentleman shall become altered, I cannot bring myself to imagine that he would wilfully have countenanced so much misrepresentation and ignorance.

I am, Sir,
Your obedient servant,
EVORY KENNEDY.

Lying-In Hospital, Dublin.

ON THE USE OF

THE STETHOSCOPE

IN THE DETECTION OF PREGNANCY, &c.

By DAVID C. E. NAGLE, A.M., M.B.,
Trinity College, Dublin.

(*Concluded from page* 400.)

AWARE of the almost unlimited degree of confidence which medical men are apt to repose in the opinions of Laennec, I apprehend that his arguments on the controverted question, "What is the site of the soufflet?" will be deemed by many entitled to very respectful consideration, notwithstanding his want of experience in the study of the phenomena afforded by gestation. To those arguments I shall now take leave to direct the attention of the reader; and whilst I am endeavouring to point out their fallacy and insufficiency, I shall at the same time be submitting to the profession my own views of the matter, without, however, expecting more attention to them than the proofs I may adduce will warrant the practitioner in considering them entitled to.

Laennec scientifically reduces the question into the form of a disjunctive proposition, which, however, he does not render sufficiently adequate or comprehensive. He is, besides, infelicitous in his mode of arguing, from the remotion of all the parts but one to the position of that one. This infelicity we must attribute to his inexperience in the study of obstetric auscultation, and not, by any means, to either a deficiency of talent or a want of candour; for his master mind could never deliberately condescend to resort to sophistry in his laudable efforts for the establishment of so grand and useful a principle in diagnosis. "The only arteries," he says, "in which the sound in question can be supposed to be produced, are the hypogastric, iliac, and uterine; if the two first were the site of it, we ought to hear it on both sides of the uterus at once, or alternately, which is not the case."

Now, with all possible deference for his opinion, I have proved, and I hope satisfactorily, that it is the case—that we do hear it on both sides of the uterus at once, or alternately; and of this any one, who has the tact of examining adequately, can easily satisfy himself. I have, indeed, seldom failed in finding it on both sides at once, exactly in front of the superior anterior spinous process of the ilium, opposite which, nearly, the uterine arteries are given off by the internal iliacs. I would take the liberty of putting the argument thus. If the two first were the site of the murmur, we ought to hear it on both sides at once, or alternately: but we can so hear it; and I therefore respectfully submit, that we are warranted, even by his own mode of reasoning, to conclude, that the two first may be the site of the soufflet. The legitimacy, at least, of this inference, no one, I believe, will be disposed to question. The next part of his disjunctive proposition he thus expresses:—"If all the uterine arteries yield it, we ought then to hear it in different parts, and several at the same time." As he does not conclude the argument, I shall endeavour to do so; and, I think, it will be fairly expressed in the following manner. If all the uterine arteries yield it, we ought then to hear it at once over every part of the uterus: but I am borne out by experience when I assert, that we cannot, at any time, hear it over every part of the uterus at once; and, therefore, it may be fairly inferred, that *all* the uterine arteries cannot be the site of the murmur. Laennec comes to the following conclusion:—"What seems to me most probable is, that it exists in the chief artery distributed to the placenta." The incorrectness of this opinion I shall endeavour to prove by the following mode of stating my objections.

If the soufflet exists in that part alone of the chief artery which is distributed to the placenta, we can have it only where there is, or lately has been, a placenta. But Corrigan's case proves the certainty of its existence where there is, or lately has been, NO placenta; and, therefore, I feel that I am justified in drawing this inference, that it does not exist in THAT part of the artery which is distributed to the placenta. Now, let us consider if it can have its proper site in the *trunk* of that vessel. Well, if it

exists there, we should hear it over the course of the trunk of that artery; but I imagine I have fully demonstrated that it takes exactly that course from below, upward and inward, towards the median line; and surely I may, without incurring censure, feel myself justified in asserting the possibility, nay certainty, of its existence in that part only of the artery. Besides, when the soufflet is at all discoverable, I never yet was disappointed in finding it over the point nearly where the lateral uterine artery takes its origin from the internal iliac; and I feel that I am not hazarding a rash opinion when I submit, that we can detect it when there is no placenta, if the uterus be enlarged by disease; for we know that the chief uterine arteries are greatly distended, not only during pregnancy, but whenever the size of the uterus is much increased by any morbid condition. Another proof of its existence in this part of the vessel chiefly, is afforded by the fact, that whenever we examine for it the lower part of that artery, the murmur is found to be confined to a narrow, but not to a short space, and gives the idea of its proceeding from a LARGE vessel; but as we move the cylinder upward and forward, it gradually becomes more diffused, as the trunk gives off its first large branches, and insensibly dies away towards the commencement of the vessels with narrow calibres, that is, towards the points of anastomosis, with the corresponding branches of the opposite side. I have no doubt that the soufflet may exist in the external iliacs also; for I have repeatedly traced it "from a point a little above the superior anterior spine of the ilium in a line, taking, from within outward, directly the course of the external iliac, even to Poupart's ligament; and, in the upper part, affording the perception of a sound deeply seated, but gradually becoming more superficial as we approach the ligament. Moreover, we can recognise the murmur to be produced by a vessel of large calibre, which could never be the case if it were confined to the vessels alone which run into the placenta. I feel that I shall not be presuming too far, in thinking that part of Dr. Kennedy's paper not perfectly correct, which supposes that change of position will, during the gravid state of the uterus, remove all pressure on the bifurcating parts of the common, or, at least, internal iliacs, and those branches of the middle hæmorrhoidal, which are given off to the lower part of the uterus. And experience ought to teach us, that the resonance will extend a considerable way from the point of obstruction, no matter of what nature the tumour may be which would press upon these vessels; I therefore put it to the judgment of every rational practitioner, whether we are justified in pronouncing a female pregnant, merely because we hear in the pelvic, iliac, or lumbar regions, a distinct or prolonged murmur. Certainly not, if what I have been stating be at all entitled to any credit.

The impossibility of the murmur being seated either in the comparatively small vessels which run INTO the placenta, or in those that pass through the parietes of the uterus UNDER the placenta, as Drs. Kergaradec, Kennedy, and others, would have it; and the certainty of its having its site in the large vessels, more especially in the enlarged trunk of the lateral uterine arteries, fully justify the inference that the soufflet is not liable to be affected in its QUALITY by the life or death of the fœtus in utero, as Dr. Kennedy would have us to conclude. This being a question of paramount importance, and one in which I happen to be diametrically opposed to his view of it, I shall now proceed to the consideration of the subject; and that I may the better enable the reader to form his own opinion, I shall endeavour to lay before him, as succinctly but as fairly as possible, Dr. Kennedy's sentiments on the point at issue.

In the last volume (5th) of "the Dublin Hospital Reports," page 267, he states, "another advantage of importance we derive from the placental sound, is its assisting us in pronouncing on the life or death of a fœtus in utero." And again, p. 269, "it affords us an indication of the death of the child, viz. either by ceasing entirely after having been previously heard, or having its character altered from the continuous murmur, with its lengthy sibilous termination, to an abrupt, defined, and much shorter sound." In opposition to this, I can assert with a confidence not over-weening, but, I presume, not ill founded, for I derive it from considerable experience, that neither change NECESSARILY follows on the death of the fœtus in utero. The murmur, which we most frequently meet with when the child is alive, is not the "continuous one with its lengthy sibilous termination:" and even when the child is dead for weeks, we can hear the same description of murmur we usually meet with when the fœtal pulsations are most energetic. So it was in the case examined, as I mentioned, by Surgeon Robinson and myself.

In another part, p. 247, Dr. Kennedy says, "The circulation in the mother and maternal part of the placenta being independent of that of the fœtus, we can understand how a phenomenon produced by the former should exist when the latter has ceased. From this we might be led to expect, that the sound should exhibit the same characters, whether the fœtus be dead or alive; but in doing so, we should fall into error." With great deference for Dr.

Kennedy's opinion, I really cannot avoid feeling that he does not adduce a single good or satisfactory argument in proof, that "by doing so we should fall into error." He gives, it is true, a case, p. 246, in which the funis, he was INFORMED, had protruded an hour before his visit; the pulsations in it were observable at the time of its protrusion, but ceased shortly after. No fœtal heart could be heard by him, but the placental sound was, however, distinctly perceptible; "it was full but shorter, more abrupt in its termination, and wanting the sibilous whiz, characteristic of the perfect utero-placental circulation. The incongruity of all this must strike the least observant. He first gives it as his opinion that the maternal and fœtal circulations are QUITE independent of each other; and because, in the case alluded to, the fœtus happened to be dead for certainly not more than half an hour, the maternal circulation should, indeed, be so strangely altered in that short space of time, as to change altogether the character of the soufflet. But Dr. Kennedy, when adducing this sort of case in support of his doctrine, never reflected that he knew not the character of the murmur previous to the death of the child. He did not examine it; and because he, on his examination of it when the child was dead, found it to have a particular character, it must therefore, of necessity! have had a different one at a time when he had no opportunity of ascertaining whether it had or not—"credat judæus apella." By such an ingenious mode of reasoning, he would certainly be going far towards establishing the validity of the "post hoc, ergo propter hoc" mode of reasoning; but I imagine it will not gain over to his opinion many converts from among the intelligent, such as I am gratified to find, my "native land" can at present boast of in the several departments of the medical profession. In opposition to the inference he would have us draw from such a description of case, I have given one p. 398, where the child was supposed to have been dead for three weeks, and the placenta was described to have been quite small and impoverished; yet Mr. Robinson and I detected a perfect soufflet, such as we ordinarily hear when the fœtal circulation is most perfect; and, perhaps, after much pains-taking industry, it would not be arrogating too much for either of us to say, that we could not be deceived in a matter which really was attended with no great difficulty.

From the case he gives, p. 246, Dr. Kennedy deduces an inference, in the validity of which I at least cannot concur, namely, that "to produce the perfect soufflet, it is necessary the blood should also traverse the placenta itself." By this he would induce us to suppose that, because in the case he presented to his readers, he found the child to be dead for scarcely more than half an hour, the maternal circulation must necessarily have ceased in the placenta; whereas he admits that circulation to have no connexion whatever with that of the fœtus. Space will not permit me to follow Dr. Kennedy through the several statements he makes: but I incline to think that the very best refutation of his doctrine will be found in his own paper, which, I regret to think, will scarcely stand the test of serious examination.

As it strikes me, and I say it without meaning the slightest offence, he mistakes altogether the principle on which the QUALITY of the soufflet depends. He supposes its character to be determined by the circulation through the placenta of the maternal blood, modified by the life or death of the fœtus. I would respectfully submit that the character of the soufflet depends exclusively on the *quality* of the maternal circulation, such as the strength, quickness, or slowness of the pulse, and on the diameters of the conduits through which the blood has to pass; and, consequently, that it has no necessary dependence on the life or death of the fœtus in utero, and, therefore, not to be taken in any case as a sign for us to form our diagnosis by. No one will deny that the murmur is perfectly synchronous with the maternal pulse. When the pulse is quick and weak, the natural murmur will be short or abrupt, as it coincides with the interval between each two successive pulsations, always commencing with the incipient state of each beat at the wrist. Therefore, a short or "abrupt" murmur, essentially depending on such a principle, should not be considered as a criterion indicative of the vitality or non-vitality of a fœtus in utero; for it does not necessarily follow, because the fœtus is dead, that the maternal circulation must invariably be quick. It sometimes happens that even a quick pulse, if it be strong, will produce the "continuous murmur," without this having any, the least, connexion with the vitality of the fœtus.

When the maternal pulse is slow, and not very strong, the murmur will, in general, be lengthened, loud at the commencement, and gradually decreasing towards its end, or the beginning of the next pulsation. Even when the interval between each two consecutive pulsations at the wrist of the mother is long, the murmur may be abrupt, particularly if the maternal circulation be not strong or excited. For instance, if I suppose the interval between each two consecutive pulsations to equal six seconds, the duration of the murmur may equal only three, four, or five seconds. In any of these cases, Dr. Kennedy would designate it "abrupt," be-

cause it did not exactly continue for the six seconds, or, in other words, coincide critically with the interval between each two consecutive pulsations. I felt the importance of paying to this soufflet an attention so particular and persevering, that my experience fully bears me out in the assertion, that we cannot, whether the fœtus be alive or dead, find it to retain any decidedly marked or permanent character, with reference to quality or duration. It is extremely variable. Whilst we are, during our examination, admiring, perhaps, the harmony and regularity of recurrence between each two consecutive murmurs, our admiration is often suddenly converted into a pleasing astonishment at the loudness and continuous intensity which they unexpectedly assume. It is not easy to account for those occasionally-increased murmurs in utero-gestation; but perhaps we should not be far from the truth, by attributing them to the streams of blood endeavouring to force their way through their wonted channels, of which the diameters may be at times a little more than usually decreased by various causes, even by the fœtus assuming a new and convenient position in the womb; or, independent of the latter, to a moral excitement in the mother, giving an increased momentary impulse to each successive column of her blood.

But why do I dwell upon this murmur? simply, because I conceive it to be a sign of paramount importance to the discriminating physician in forming his diagnosis. Though I cannot bring myself to consider it an unequivocal sign of pregnancy, I am ready to admit it as perhaps the least equivocal of the equivocal ones; and its existence, taken in conjunction with the history of the case, is calculated to raise in the mind of the reflecting practitioner a strong suspicion, at least, of impregnation. Our attention being directed to the character of this murmur, we shall be able to infer how fallacious is that theory, which would have us suppose that the quality of the soufflet should be taken as an indication of the life or death of the fœtus. This soufflet Dr. Kennedy supposes to be produced, either by the blood passing through the arteries of that part of the uterus to which the placenta is attached, without passing into the placenta itself; or "that it may greatly depend on the passage of blood through those uterine vessels which pass INTO the maternal portion of the placenta." In the first case, the sound would be occasioned merely by the pressure of the placenta on the vessels. Now, if this were the cause of the murmur, which I deny, how could the death of the fœtus so affect it, as to produce, all at once, so important a change in its character?—unless he supposes that dead matter becomes immediately far lighter than living matter; and, therefore, that the former weight upon the placenta, being now necessarily diminished by the death of the child, the pressure previously made upon the arteries running under the placenta, must also be decreased. But, on the other hand, if the murmur, according to him, "may greatly depend on the passage of blood through those uterine vessels which pass into the maternal portion of the placenta," I would venture to say that, even so, the death of the child could not induce such an instantaneous change in the quality of the soufflet, if, as he admits, the two circulations are perfectly independent of each other. I beg it will be considered that I mean this latter argument only as an "*argumentum ad hominem*;" for I cannot agree in opinion with some others, that the two circulations are totally independent of each other; that they are connected by absorbents at least, I am scarcely wrong in supposing; and on this account I think it a very fair inference to consider, that when the fœtal circulation has ceased for some time, the circulation in the maternal portion of the placenta should also undergo some alteration, and consequently the murmur, if "it depend greatly on that circulation," exhibit, in like manner, some modification. But experience has fully proved to me, at least, that it does not undergo the slightest alteration in quality; and I, therefore, take it as another strong proof that the soufflet is not owing to the "passage of blood through the chief artery distributed to the placenta;" and also that its character is not, necessarily, liable to be affected by the death of the fœtus in utero.

I should, indeed, be delighted if the profession could have so undeceptive a diagnostic in the character of this murmur; and with the view of ascertaining this important point, I had frequently, before Dr. Kennedy's paper made its appearance, or I had any means of knowing his ideas on the subject, investigated the matter as critically as possible. The moment I heard of his views, my experience warranted me in denying totally the validity of his opinion; and I recollect to have told Dr. Kennedy, in the presence of some of the pupils of the hospital, that "there was in it at that moment a patient whose child was dead for some time, yet that he would find the soufflet prolonged and continuous." In support of the opinion which I am thus venturing to offer respecting the value of the soufflet as a diagnostic, I could adduce many cases, in addition to that described in p. 398 of THE LANCET; but I shall confine myself to one instance more, which, I am induced to think, will be quite decisive on the point. We had not very long since in the hospital a patient with a syphilitic taint; her child, ausculta-

tion proved to be dead, and that this was the case for some time its excessively putrid state was well calculated to show. Yet in this case also, Dr. M'Effer and Mr. Neville, both pupils in the hospital, were so satisfied of the existence of a full, prolonged, and, at times, continuous murmur, that they considered it a decisive corroboration of my opinion, in which I had the satisfaction of their concurrence on more occasions than one. In support of my view of this question, I might also adduce the testimony of some of my fellow-students at the Meath Hospital, where auscultation is carried to great perfection indeed, under the encouraging and judicious guidance of its eminently successful physicians, Drs. Graves and Stokes.

That auscultation should be deemed the only unequivocal sign of pregnancy, has been denied by some, apprehensive of placing, by such a concession, "their knowledge of practical midwifery in a very questionable shape." But in opposition to their doctrine, I not only am ready to concur in opinion with my respectable young countryman Dr. Ferguson, but willing to risk even my "knowledge of practical midwifery" on the hazard of the declaration, that auscultation supplies us with the only unequivocal sign of utero-gestation, in as far as we can detect by it the pulsations of the fœtal heart, which banishes all doubt and gives our profession, in this instance, all the certainty of demonstration. What other unequivocal sign is there? Not a single one can any man even pretend to adduce. Here then the stethoscope supplies us with a paramount advantage; and I have no doubt, that, in any case where a fœtal heart pulsates, the ear, which is sufficiently practised to accurate auscultation, will experience but little difficulty in its detection. Should the auscultator fail of hearing distinctly the pulsations themselves, their resonance, at least, will apprise him of the heart's existence; for even in cases where there was a very great accumulation of liquor amnii, the bare resonance of the pulsations enabled me to determine the precise point under which I could detect, most distinctly, the heart's action.

The inexperienced observer is liable, at times, to confound the pulsations of the fœtal with those of the mother's heart, as the following case will not only prove, but also show the most satisfactory and obvious method of drawing the distinction. On the 9th instant, I was informed, in one of the "sick wards" of the hospital, by Surgeon H. Alcock, that there was in it a female in the seventh month of her pregnancy, and that he was informed the fœtal heart was audible, but faintly so. In order to satisfy myself I had recourse to auscultation, and heard below the umbilicus a feeble pulsation resembling, in some degree, that of a slow fœtal heart; but immediately it struck me that it was not the action of an infant's heart. As I could not satisfactorily determine the point at once by a comparison with the mother's pulse, which was very rapid, I removed the cylinder to the præcordial region, when all doubt was instantly dissipated by the perfect identity of the rhythms. Should any difficulty arise to the inexperienced, in discriminating between the rhythms heard at such remote points, the observer has only to move the cylinder gradually from the lowest part of the abdomen, where the pulsations are detected, upwards towards the mother's chest, listening attentively during the ascent of the cylinder; and the slightest permanent discrepancy in the rhythms, determines that those in the abdomen are not produced by the action of the parent's heart, which, we know, can sometimes be heard as low down as the hypogastric region. The double beats, and the rapidity of the fœtal heart's action, determine, in ordinary cases, the question without any difficulty, for in general they are not only double those of the mother's heart, but, in some instances, considerably more than double; as in the first of the twin cases given in a former paper in THE LANCET, where I mentioned that the pulsations in one fœtus varied from 160 to 170, whilst those of the mother amounted only to 60 in the minute.

These are not the only advantages afforded by the stethoscope in the practice of midwifery. It further supplies us with the easiest and only means of ascertaining the presence of twins, as I have before pointed out; and experience authorises me to say, in opposition to any objections that may have been adduced by those unpractised in accurate stethoscopic observations, and who reason only from the CONVENIENT inspection of casts and plates, that we can in most cases determine by it the nature of the presentation. In two cases where the contrast was very striking, I have already shown the possibility of arriving at this marked and unquestionable advantage; and lately in a case where the kind of presentation was doubtful, auscultation alone enabled me to decide that it would be that of the breech. This advantage afforded by auscultation all must admit to be a most desirable improvement in the practice of midwifery; as, in addition to other benefits, we shall not, at any time, run the risk of rupturing membranes prematurely, and thus rendering dangerous, as well as tedious, the accouchement of our patient. Surely it is neither fair nor candid in men to argue from their own inexperience in the employment of the stethoscope to the incapability of

others more practised in its use; or to conclude that, because one case of extreme difficulty may present itself, we should, therefore, despair of deriving any advantage from it in hundreds of others. I hope, for the sake of humanity, delicacy, and science, the reasoning of such *philosophic* and practical men will have but little weight with the judicious and unprejudiced portion of the profession.

If the great importance of the subject did not appear to me a sufficient justification for so lengthened a paper, I should feel myself called on to apologise for trespassing so much upon the attention of the profession. But the discussion, should it be productive of no other advantage, may, at least, be the means of inducing some persons, more competent and better supplied with the proper facilities, to rouse themselves from their inaction, and "let slip" inquiry for the discovery of the much useful information as yet acquirable in this department of our profession. To me, indeed, it is matter of regret, that, in the views I have taken, I should be under the necessity of differing so widely from the opinions of men pre-eminently distinguished. In doing so, I hope I have not transgressed the limits of legitimate discussion; and to the unprejudiced portion of the profession I shall not only leave the decision respecting the questions in dispute, but to that decision I, at least, am ready to submit with the utmost deference and befitting respect.

33, Trinity College, Dublin,
Nov. 25th, 1830.

MEDICAL JURISPRUDENCE REDUCED TO THE CAPACITY OF A BAKER.

An Attorney-Coroner, on being elected for a small district of a large county, applied to a neighbour in the medical profession to learn what work he should read, saying "that he supposed he ought to know a little of medical jurisprudence. The Doctor spoke of Paris's and Fonblanque's work. "Oh," replied the attorney-coroner, "I have seen that book, it is too deep for me; it is deeper than Garrick." Beck's Elements were then mentioned as being more explanatory and easier of comprehension. "Ah, then," exclaimed the new-made Rhadamanthus, "*will you lend me the book for a few days?*!!!!

THE LANCET.

London, Saturday, January 8, 1831.

The period is not far distant when our medical, as well as our political, institutions, will feel the ameliorating influence of the intellectual revolution, which is now in progress from one end of Europe to the other. The voice of philosophy in France, the acclamations of triumph in Belgium, and the humble whine of concession to the stern dictates of necessity in broken-hearted Britain, proclaim the dissolution of systems in every department of life, whose existence was protracted, unfortunately, too long for the happiness of mankind.

To whatever point, indeed, of the social world we turn, the sight is gratified by the prospects of futurity, and the ear saluted with sounds of promise, which every movement of the mighty mass asserts the advent of a novel and a better era in the melancholy history of man. The various and rapid phenomena of the moral horizon, predict in short, the succession of an age of reason and intelligence, to an age of faith and credulity. There is, in fact, no mistaking these obvious signs of the times, and what is of nearly the same importance, their meaning may at length be promulgated without the fear of persecution. Principles which were silently entertained, or published with timidity by an order of men, whose superior genius placed them in the predicament of being born before the world was capable of benefiting by their speculations, have accumulated to such an overwhelming extent, and have become so generally diffused, that, from being enshrined in libraries open only to the eye of the curious, they have become the ordinary topics of conversation among the humblest classes of men, and the judicious regulators of their opinions.

We cannot, we conceive, render a more meet homage to this spirit of regeneration, whose slumbers we have sought to awaken from the first moment of our existence as public journalists, than that of pointing out the obstacles which so long impeded its progress in the medical profession. Already have we devoted, and not unprofitably we hope, much of our time and space to an ex-

posure of the numberless abuses which have been generated by the monopolising medical colleges of this metropoli . The picture, however, would not be complete were we to omit throwing in the light and shade furnished by the system of medical government and education in the universities of the sister kingdoms. When the ground is cleared of the rubbish by which it is now occupied, it may be considered how it shall be best filled up, and there will not be wanting architects to adapt plans to the circumstances of the site of a new and improved structure of economy. It would, however, be worse than doing nothing to attempt to patch up the old edifice, whose rottenness defies the hand of renovation. Such is certainly not our design in taking to pieces its corrupt materials. For the purposes of an experimental demonstration of its noxiousness it has been kept together long enough by the fostering embraces of the parasites who, like the ivy on the ruin, draw nourishment from the impurities of the tottering structure to which they pertinaciously cling. In this country it has produced nothing but ignorance, dissension, and a humiliating contrast with the condition of the scientific establishments of other nations, whose superior schemes of medical education have been supplying us with discoveries and improvements in the healing art for the last half century. We might, in the present instance, take any one of these plans of instruction to measure the inferiority of our own, and to demonstrate the necessity of their removal; but we prefer taking our illustrations from our own institutions, as better understood by most of our readers.

For this purpose the schools of Edinburgh and Dublin may be compared with each other with advantage, not for the purpose of giving an envious superiority to one over the other, but to deduce from the contrast the necessity of some better system than either of them supplies for the instruction of medical pupils. Apart from the conclusions to which a comparison of these establishments must add, their very different success forms a subject of interesting inquiry. With many local advantages in its favour, the school of Dublin has as yet kept at a respectable distance behind its northern contemporary. Not only has the Edinburgh school supplied its own market with doctors, but it has also furnished Ireland and England, we may say, with the great mass of their practical physicians. No later than the last graduation at that city another proof was added to the truth of Dr. Duncan's statement, which informs us that the number of Irishmen who took degrees in medicine in the University of Edinburgh exceeded considerably the number of Scotchmen who graduated there during the last fifty years. To persons unacquainted with its true cause, this simple fact must appear perfectly unaccountable, when they reflect that Dublin is blessed with the presence of a university as well as the capital of Scotland, besides possessing nearly a tenfold proportion of all those materials which are supposed to be essential, or at least useful, to the success of a great medical seminary. One, indeed, of the first things which strikes us in looking over the history of the Edinburgh University, is its rapid elevation to eminence, contrasted with its original destitution of many at least of those attributes deemed necessary to the very existence of a school of medicine. Not many years back, compared with some of its contemporary universities, there was neither a professor of medicine in the University of Edinburgh, nor a single hospital for the treatment of disease. Yet since the establishment of its medical schools it has been filled by a greater number of professors of eminence, and attended by more numerous classes of medical pupils, than any one of the British universities. Success so extraordinary cannot be fairly imputed to accident alone: the constitution of the school, indeed, affords the most satisfactory solution of the problem, and explains in the clearest manner its unexampled prosperity. An outline, therefore, of the economy of the Edinburgh school will tend to elucidate the object we have in view.

In the first place, then, the professors of this school derive from it no permanent salaries; their chief support being obtained from the fees of their pupils. Secondly, they are not elected to their offices by the University, but by its patrons, the town council or corporation of Edinburgh, who, with the exception of one individual belonging to the Corporation of Surgeons, are all unprofessional men. Thirdly, the fees of the pupils and expenses of graduation, were regulated, not by the professors themselves,

but by the body whom we have just mentioned. Fourthly, the possession of a previous degree in arts, was not included among the essential qualifications for graduating in Edinburgh, so that a considerable economy of time and expense was secured to the students of this school. The power, in fact, of the professors over the regulations of the school, was at all times extremely limited; and by the result of the late litigation between them and their patrons, that little has been diminished to nothing.

Such were the principal features of the Edinburgh school, which crowded its halls with pupils from Ireland and England, and which were so far productive of the best consequences. We may now proceed to notice the operation of these several causes of its superiority.

Impolitic as it may appear, at first sight, that a corporation composed for the most part of mercantile men, should preside over a scientific institution, and appoint its professors, the practice, besides being justified by the event, will bear the test of theoretical examination. Assuming the fact (and in the present instance we may safely do so, their interest and their duty having coincided) that this unprofessional body of men was determined to do justice to the University, they were at least exempt from one of those sources of abuse which has been found to preponderate against every other qualification of electors to universities. Having little or no connexion with medicine, and judging of course of their fitness for office exclusively by the public proofs which they gave of their merit, electioneering jobs were completely excluded from the performance of their duties. They had, in fact, only to look around, when a vacancy in the University occurred, and select the individual who evinced most talent, and had acquired the greatest share of reputation in the cultivation of that science, for the tuition of which he was required. In general, we believe, this duty was conscientiously performed by the Town Council of Edinburgh, much more honestly, at least, than by any other body of scientific or literary electors belonging to universities. By such an arrangement, therefore, one great source of corruption was cut off, and a succession of able professors secured to the school. In this comparative approval of such a system of appointing professors, we do not mean to assert that it is the best that could be devised; our observations merely go the length of demonstrating by its results its superiority over every other scheme of election in present use. To every unbiassed mind, the system of election by public examination, must, of course, appear the best. While the former mode of filling the chairs of the University of Edinburgh supplied it with eminent men in their respective departments, the mode of paying them extorted exertion. Having no salaries independent of labour, they were of necessity compelled to exert themselves in order to have pupils. "In every profession," says Adam Smith, "the exertion of those who exercise it, is always in proportion to the necessity they are under of making it. This necessity is always greatest with those to whom the emoluments of their profession is the only source from which they expect their fortune, or even their ordinary resource to subsist."

The important principle of action involved in these remarks, was fully developed in the conduct of the Edinburgh professors, whose fortunes depended on the assiduity they displayed in teaching the science they professed. The arrangement which thus precluded indolence from the discharge of their duties, prevented them, at the same time, from extorting exorbitant fees from their pupils, for they had not the power of compensating the deficiency of permanent salaries by exactions on the students, as is usually the case in those places where such power is conferred. Were this power, indeed, possessed and exercised by them in the manner in which avarice too often suggests, it would have completely frustrated every other advantage of economical arrangement which we have noticed in the University of Edinburgh. It also served, we conceive, materially the interests of the University, that its scheme of education was not entirely projected by the "Senatus Academicus." A literary and scientific body of that kind might be inclined to expect (particularly as their taste and interest were agreed on the point) too high qualifications from their pupils—to impose too great a weight of learning on them, in order to lighten their pockets; but the great mass

of medical students are unable in these countries to comply with such expensive demands, or to devote so much time as might be required of them to a lengthened course of instruction. Comparatively superior, however, as these several ordinances were, they would never have enabled the University of EDINBURGH to rise to that degree of eminence, had their operation not been assisted by the perverse laws of the College of DUBLIN and of the English universities, which we shall take an opportunity of contrasting with the constitution of the university just described, in the next LANCET.

SINGULAR CASE OF WOUND OF THE EYE.

In one of the late numbers of *Graefe u. Walther's Journal* we find the following case by Dr. Salomon of Schleswig :—

H. S., ætat. 24, was on the 3rd of July, 1824, shot in the right eye, and as the accident had happened during bird shooting, it was supposed that a small shot had entered the eye. Dr. Salomon found the man about an hour after the accident with violent pain in the right eye, and difficulty of moving it; the upper eyelid was red and much swelled, so that the patient could hardly open it; and nearly in its middle, about three lines from the edge, there was a small wound of the size and form of a common grain-shot; the grain had passed through the eyelid, and entered into the globe at the external margin of the cornea, where there was a small aperture, through which part of the iris had prolapsed; the conjunctiva of the sclerotic was very much injected; the cornea was transparent, but rather unusually prominent; of the iris nothing could be seen on account of the anterior chamber being filled with blood. There was a great discharge of tears, and sight was of course completely destroyed; in other respects the patient was pretty well; there were no cerebral symptoms, &c. He was ordered to be largely bled, to have ice applied to the wounded eye, and to take a saline aperient; the right eye was closed with adhesive plaster. On the next day the inflammation had become more intense; the conjunctiva discharged much mucous serum, and rose in small vesicles; the pain was very violent and the fever high; the prolapse of the iris not altered. He was ordered to be again bled, and to continue the other remedies. On the third day the conjunctiva was still more swelled; the mucous discharge continued, but was rather more consistent. The patient was bled a third time, and besides the application of ice, a solution of the acetate of lead in a decoctum malvæ was instilled into the eye. No alteration having taken place on the fourth day, a large quantity of leeches were put round the eye, as the general state of the patient did not admit of his being bled a fourth time. Besides the saline aperient, calomel and frictions of mercurial ointment round the eye were ordered. Under this treatment the pain had, on the seventh day, entirely subsided, but the swelling of the conjunctiva was not in the least diminished, the prolapse of the iris had quite disappeared, and the eye appeared like a piece of flesh, similar to what is observed in gonorrhœal ophthalmia, Dr. Salomon now scarified the conjunctiva and ordered the eye to be fomented with a solution of the oxymuriate of mercury and the tincture of opium. This had the desired effect, and the swelling of the conjunctiva had on the tenth day so much subsided, as to render the prolapsed iris again visible, which being found to have become detached was accordingly removed; the wound of the cornea and that of the eyelid were completely closed; the anterior chamber of the eye continued, however, to be filled with blood; of the foreign body nothing could be seen, and sight was still completely suspended. In order to promote resorption a large blister was put on the neck, the patient well purged, and the external application of sublimate and the tincture of opium continued. On the sixteenth day the first signs of resorption were observed at the upper part of the cornea, through which the iris began to shine, and after a few days more the patient began to distinguish light. On the twenty-fifth day the extravasated blood was almost entirely resorbed, when all on a sudden, without any perceptible cause, a slight nebula was observed behind the pupil, which gradually increased, and on the twenty-ninth day, filled the posterior chamber, so as to produce again complete blindness. This evidently proceeded from cataract, owing probably to a lesion of the capsule. The patient was now ordered large doses of calomel, and mercurial frictions, and to instil a weak infusion of belladonna. On the fifth day of this treatment, salivation began to take place, and the mercury was accordingly omitted, but at the same time a very remarkable change became visible in the cataract; radiated fissures were seen in it, and after a few days the lens fell into several fragments, a few of which were deposited in the anterior chamber. From this moment absorption went on very rapidly, and on the forty-fifth day no trace of any fragment of the lens could be discovered. At the lower portion of the anterior chamber only a small whitish point remained, which after some

time was discovered to be the grain surrounded by flocculent lymphatic matter. Sight was completely restored, and the patient complained only of much irritability in the left eye, on account of which he was ordered to continue the external use of the opiate solution.

At present, six years after the accident, the grain is still in the eye surrounded by whitish semi-transparent lymph, and without causing the least inconvenience; the cicatrix of the cornea is not transparent, but being at its margin it does not hinder sight materially; the pupil is of an oval form; the iris is perfectly sensible and not discoloured.

SUCCESSFUL CASE OF SYMPHYSEOTOMY.

The account of this case is extracted from the "Compte rendu des Tr. de la Soc. Méd. de la Moselle," and will be read with interest, as it shows the extent to which nature is capable of healing such lesions as are necessarily produced in symphyseotomy. As to the surgeon or accoucheur who performs it, we doubt whether we can praise him, as in our opinion there do not exist any cases of labour in which this operation is necessary, or in any way capable of facilitating delivery.

J. M., ætat. 23, of a robust constitution, and in her first pregnancy, had been in labour for about forty-eight hours, when M. Stock, of Creutzwald (Dep. de la Meuse), by whom the case is related, saw her for the first time; the waters had escaped at the beginning of labour; the child seemed to have been dead for some time; the external genitals were swollen and very painful, and a great quantity of meconium and bloody mucus was discharged from the vagina. The uterine contractions had almost entirely ceased. The neck of the uterus had completely disappeared, and the os was fully dilated; but the head, which presented with the occiput, was by far too large for the dimensions of the pelvis; the antero-posterior diameter was only two inches and a half in length, and the branches of the pubic arch were so near one another, that hardly two fingers could be introduced into the vagina, so that the application of the forceps was out of the question. As the child had for some time ceased to give any signs of life, the head was perforated, and after the evacuation of the brain, the blunt hook and the index were alternately used, in order to bring the head down the pelvis, but without any effect. These various attempts lasted for several hours, and exhausted the patient to such a degree, that it seemed advisable to give her a few hours' rest, after which the tractions at the head were recommended, but without any better effect, even after a great portion of the bones of the head had come away. Symphyseotomy was now decided as the only means to deliver the woman; a catheter was introduced into the bladder, and after the incision through the skin and subcutaneous tissue of the mons veneris, the cartilage of the symphysis was cautiously divided. By the operation, the canal of the pelvis is reported to have become enlarged by an inch more; for the operator was able to introduce his hand into the vagina, and after having seized the remaining portion of the head, succeeded in bringing it down; the trunk also soon followed, and delivery was, without any further difficulty, terminated by extraction. The wound was dressed with sticking-plaster and a uniting bandage, and healed in a very short time, without any particular treatment, except that the patient was kept low, and as quiet as possible. The lochial discharge was very profuse, and of a purulent kind, accompanied by hectic fever, emaciation, &c., which symptoms, however, within a short time, spontaneously ceased. About a twelvemonth after the person was in very good health, except that she, since the operation, had been affected with incontinentia urinæ.

MEDICAL FEES AT NEW YORK.

The following table, extracted from a "Sketch of the State of Medicine in America," by Dr. Black of Bolton, will probably be interesting to our readers. Some of the fees appear to us to be not only absolutely, but proportionally, too high; and the distinction between different operations in reference to the degree of skill which they require, seems to be carried much too far, and not very well managed; thus, probably, as much skill, and certainly more time and trouble, are required in the operation for harelip than in that of depressing cataract; yet the remuneration for the former is only one-fifth of that for the latter, which is no less than 26*l.*,* much too large a sum for ordinary occasions.

	DOLLARS.
Verbal advice.....from	0 to 15
Letter of advice.....	10 to 15
Ordinary visit.....	0 to 2

* Dr. Black reckons the dollar at four shillings and two-pence.

	DOLLARS.
Consultation ditto	5
After visits, each	3
Night visit	7
Visit, per mile, distance	1½
Visit to Staten Island	10
Double in winter or in a storm.	
First visit in epidemic or other diseases, where personal danger is apprehended.	5
Each succeeding, under same circumstances	3
Vaccination	5 to 10
Each dressing of wound	1 to 5
Cupping	5
Bleeding in arm or foot	2
Ditto in jugular vein	5
Dressing blister	1
Scarifying eye	5
Puncturing œdematous swellings	2
Inserting seton	5
Ditto issue	2
Visit in haste, to be charged double	
Detention, per hour	3
Ditto per day	25
Introducing catheter	5
Ditto ditto each succeeding time	2
Ditto in females	5
Extracting calculus from urethra	20 to 30
Reducing simple fracture	10 to 20
Ditto compound fracture	30
Ditto dislocations	5 to 20
Ditto ditto of the hip	30 to 50
Reducing prolapsus ani	5
Ditto hernia	10 to 25
Amputation of the breast	50
Ditto leg	50
Ditto hip or shoulder	100 to 150
Ditto finger or toe	10
Ditto penis	20
Extirpation of testis	50
Ditto of eye	100
Ditto tonsils	25
Ditto tumour	5 to 50
Perforating rectum	25
Ditto nostrils, ear, vagina, or urethra	5 to 25
Opening abscess	1 to 5
Dividing frenum linguæ, or penis	3 to 5
Paracentesis abdominis	15 to 25
Ditto thoracis	50
Operation for the tic douloureux	25
Ditto for hare-lip	25
Ditto for hernia	125
Ditto for fistula perinei	50
Ditto ditto in ano	50
Ditto for phymosis	10
Ditto fistula lachrymalis	40
Ditto paraphymosis	10
Ditto wry neck	50
Ditto depressing cataract	125
Ditto extracting ditto	150
Ditto popliteal aneurism	100
Ditto carotid aneurism	200
Ditto for inguinal or external iliac	200
Ditto brachial	0
Ditto radial, or ulnar	25
Lithotomy	150
Bronchotomy	25
Trephining	100
Circumcision	10
Common case of midwifery	25 to 35
Tedious or difficult labours	36 to 60
Case of gonorrhœa	15 to 30
Ditto syphilis	25 to 100
Preparing and giving enema	2
Visit for opinion involving a question at law, and in which a physician may be subpœnaed	5
Extracting tooth at patient's house	2
Ditto at the surgeon's	1
PHARMACEUTICAL CHARGES.	
A simple prescription furnished	½
Pills, per dozen	½
Boluses, each	½
Electuaries, per ounce	1
Infusions, per pound	2
Solutions, per pound	1½
Tinctures, per ounce	½
Ointments and cerates, per ounce	½
Blisters according to size	1½ to 2½
Decoctions, per pound	2
A single medicine dispensed without visit	1
An anodyne draught	½

ST. GEORGE'S HOSPITAL.

REMOVAL OF AN INFLAMED GLAND IN AN OPERATION FOR CRURAL HERNIA.

December 27th. Sarah Smith, aged 49, was admitted at 10 o'clock A.M.; states that for the last sixteen years she has been afflicted with hernia, which occasionally descended, but that she was always able to replace it until last Friday, when it again came down, and resisted all her efforts for its return. She accordingly applied in great pain to the medical gentleman who usually attended her, who after using the taxis and giving some aperients, applied ice, and afterwards tried the tobacco enema, but without avail. This gentleman also states that on Saturday, finding all his attempts at reduction to be useless, he recommended his patient to come from Richmond to St. George's Hospital. This, however, she did not do until Monday morning, and states that without her truss she would have been unable to make the journey. She also states that on setting out, the tumour in her groin was much larger than on her admission. Her symptoms were, some pain in the abdomen, though not so severe as it was; bowels costive for the last four days; tongue furred; pulse 80, and rather

strong. It may be remarked that there was no vomiting, nor was the abdomen at all tense.

Shortly after her admission she was placed in a warm bath, and the taxis was employed.

One o'clock. The surgeons arrived, and the taxis was again employed by Mr. Babington for a short period. This gentleman immediately proposed an operation, which was objected to by Mr. Keate; however, on its being represented to Mr. Keate (by Messrs. Babington and Hawkins) that the woman had been ill so many days, that every effort at reduction had proved useless, and that she had already been four hours in the hospital, he consented.

On Mr. Keate being questioned on the subject of the complaint by a pupil, he observed that "it was a crural hernia immediately in contact with the crural sheath, and as distinct a case as ever came under his notice."

OPERATION.

Mr. Babington proceeded to make a perpendicular incision in a line with the tumour, after which he divided some cellular tissue; he then proceeded to divide the fascia, when a soft tumour came into view, which for a short time was mistaken for the hernia. The surgeons, however, were convinced of their mistake, and Mr. Babington proceeded to remove more cellular tissue, but no hernia was discernible. Mr. Keate then passed his finger as far as the crural ring, and stated he felt the intestine pressing against the ring. Mr. Babington immediately removed the soft tumour, which proved to be an *inflamed gland*, and looking round said, that the hernia had passed up and he was happy he had not to divide the stricture.

The answers made by the surgeons in a body to the pupils, were not very satisfactory; and on application to them separately to ascertain "what kind of hernia it was," one surgeon stated that it was omental, another intestinal, and a third, that "if it were intestine he saw above the gland, it was very dark-coloured," but could not give a positive answer.

A suture was introduced, a T bandage applied, and the woman was placed in bed. She was ordered as follows:—*Epsom salts, one drachm; Peppermint water, half an ounce, in the form of a draught, to be repeated every hour till the bowels are moved.* Eight P.M. *Calomel, ten grains immediately.*

Dec. 28. Has passed a bad night, no sleep; great pain in the abdomen; tongue foul; pain in the head; vomiting incessant; hiccup; pulse 100, and very weak; bowels very slightly open. *Calomel to be repeated immediately.* She also had a common injection in the course of the night.

She is now ordered some brandy and soda water to allay the irritation of the stomach.

Eight o'clock P.M. Vomiting continues; bowels not yet opened; pulse 100, weak and fluttering; the woman seems to be fast sinking. *Repet. enema com.* The brandy and soda water to be continued.

29. Eleven o'clock A.M. The woman died.

Examination 27 hours after death.

On opening the abdomen there appeared no traces of recent inflammation, there were some adhesions between the peritoneum and omentum, which appeared to have existed for some time. On directing attention to that particular portion of intestine which was the immediate object of our inquiry it proved to be *quite free*, somewhat dark-coloured for about two thirds of its circumference, but certainly not in a state of mortification; it seemed, however, not to have regained its tone; there were no adhesions between it and the sac, nor was any portion of it under the crural arch.

HOPITAL DE LA CHARITE.

VASCULAR TUMOUR OF THE WRIST—LIGATURE OF THE BRACHIAL ARTERY.

D——, æt. 36, of a vigorous constitution, was admitted on the 10th of November, with a tumour at the right wrist, which, according to his statement, had about six months ago begun to form, without ever causing any great inconvenience. It occupied the whole circumference of the wrist, was free from pain, even on pressure, and exhibited obscure fluctuation; the skin was of reddish hue, the subcutaneous veins were distended with blood, and the pulsations of the radial artery were felt very superficially. In order to clear up the diagnosis of the case, which M. Roux was rather disposed to consider as one of white swelling, an exploratory puncture was made at the most prominent part of the tumour, which gave issue to a small quantity of bloody serum, and during which, M. Roux thought he felt the instrument enter the substance of the radius, and penetrate through a tissue of small osseous laminæ. On further examination, the tumour was found to pulsate in its whole extent; on compression of the brachial artery, these pulsations ceased, and the tumour diminished in size. All these symptoms convinced M. Roux, that the case was one of vascular tumour in the substance of the bone. As to the most proper method of treatment, he hesitated between the ligature of the radial or brachial artery, but finally decided upon tying the latter. The operation was performed on the 7th of December in the following manner:—An inci-

sion, two inches in length, was made at the inner side of the middle of the upper arm, and the vessel laid bare for about half an inch; a double ligature was then passed under it by means of a director, and the artery tied, with the interposition of a cylinder of sticking-plaster between the threads and the vessel. At the moment when the ligature was applied, the tumour became less in size, and the pulsations in it ceased altogether. The wound was covered with lint, and a bladder with ice was applied over the tumour. On the following day the tumour had still diminished in size; the temperature of the arm was slightly increased, and the patient was in every respect going on well.—*Lanc. Franç.*

HOSPITAL DE BICETRE.

ASPHYXIA IN INSANE PERSONS.

It seems that in some cases of insanity, and in particular in the last stage, which is characterized by several paralytical affections, the pharynx, œsophagus, and muscles of the larynx, are also in a state of torpidity; so that, after deglutition, the bolus remains in the pharynx, and either by the interception of air, or by entering the cavity of the larynx, may cause asphyxia. The following two cases of this kind were lately observed by M. Ferras, at the above hospital.

A man, about forty years of age, had, after several attacks of mania, became idiotic, and affected with general paralysis,—such as trembling of the limbs, insensibility of the skin, stammering, &c.; the digestive and respiratory secretions were natural. On the morning of the 25th of November he appeared to be in his habitual state of relative health, and at noon or about one o'clock sat down to dinner as usual, when he suddenly and whilst eating dropped down, and was, after a few minutes, found by the "interne" without any signs of life; no spasmodic affection of the respiratory organs, or any kind of struggle, had preceded his death. On examination, the brain exhibited signs of chronic inflammation; the cavity of the pharynx contained a large quantity of half-chewed food, part of which was found pressing on the upper surface of the epiglottis, so as to intercept completely the passage of air; in the cavity of the larynx there was also a small portion of food, and the trachea and larger bronchia contained about two ounces of it. The other viscera did not offer any-thing of interest.

The same accident happened to a man of about 60 years of age, who was affected with epilepsy and mania, and after having been at the hospital for several years, had lately been placed in the "Section des Incurables." He was once observed suddenly to stop; and as the keeper thought it was one of his usual fits, he did not take any notice of him until he saw that he was quite motionless. Assistance was immediately procured; and as no cause of this sudden death could be found, it occurred to the "interne" whether this was not a case of asphyxia dependent upon the same cause as in the instance related above; he accordingly examined the pharynx, and succeeded in extracting from it and the œsophagus a large quantity of bread and other food; but it was without any effect, and life remained extinct. On examination of the body, the pharynx and œsophagus contained scarcely any food, but in the larynx almost half an ounce of it was found; the trachea and bronchia were healthy; the lungs were slightly emphysematous.—*Lanc. Franç.*

ELECTION OF AN ASSISTANT-SURGEON TO WESTMINSTER HOSPITAL.

A VACANCY has occurred in the office of assistant-surgeon to the above hospital, and Mr. G. D. Dermott, of Gerrard Street, has addressed an admonitory letter to the governors, in which he offers himself as a candidate for the situation, in the hope that an example will be set in the present instance by a public election. The following is an extract from Mr. Dermott's appeal:—

"At present medical officers are chosen for public institutions, in England, by a shameful process of intriguing after private interest. My proposal is that you place this election upon the basis of merit, just as medical elections are conducted in France. Let there be a public and fair examination of the professional merits of those who may offer themselves as candidates before competent and disinterested judges, members of the profession, or other gentlemen who may wish miscellaneously to attend.

"It has been stated that an examination, as to capability of talent or professional knowledge, would be offering an indignity to the medical candidates: but who are the men that say so? Those very individuals who depend upon their private interest, not upon professional knowledge, for their success; and who, at the sound of an examination, would retire into the dark abodes of 'Owls' and 'Bats.' The field would then be thrown open to just competition; the medical man would become hospital physician, surgeon, or apothecary, in a way most honourable and gratifying to himself; and instead of feeling that the scrutiny of his talent had insulted his merit, it would

for ever surround him with a splendour of well-earned fame, and bestow confidence in the public mind as to the treatment of patients within the institution.

"A man, on the other hand, who obtains hospital appointment by the present ferreting system, wreaths weeds for his brow instead of laurels; and I, for one, whatever may be my personal friendship and esteem for some of my brethren who are at present hospital surgeons, shall for ever regard such a process of obtaining false fame and increase of pecuniary gain, with the utter contempt which it deserves. Should you however, gentlemen, throw open the election to fair competition, I would be the first to throw down my gauntlet.

"Lastly, let me entreat you to embrace this occasion to prove to the world that you are real philanthropists; that you are advocates for liberty of science; that you wish merit and fame, like twin sisters, to go hand in hand; that you have the welfare of the institution, the welfare of the patients, and the welfare of all your countrymen, at heart."

"Westminster Dispensary,
Gerrard Street, Soho,
Nov., 1830."

ON THE LAWS WHICH REGULATE THE

MOTIONS OF THE IRIS.

The experiments of Berzelius, Monro, Jacob, and Bauer, tend materially to prove that the iris is shaped from muscular fibres, which are arranged into a circular and a radial system; the first is supposed to effect the closure; the last, the dilatation of the pupil of the eye.

More than a hundred years ago, the industrious Mery failed, after a long search for muscular fibres, in detecting their existence: the penetrating eye of Dr. Knox has been alike unsuccessful: Zinn could not discover circular fibres, and I much doubt whether he has been less happy than others in his researches. The facts which I am about to mention, may be accounted for by the contractions and dilatations of the radial fibres; or by the alternate motions of the circular and radial: as the latter are generally admitted, I follow them, although not convinced of their truth.

Both the circular and radial fibres are concerned, either in a contraction, or a dilatation of the pupil; neither the former nor the latter effect takes place in health (I cannot answer for disease), without the concurrence of both; contraction being determined by the greater action of the circular, and dilatation by that of the radial system. This is seen best when a concentrated light is introduced into a dark apartment, and permitted gradually to approach the eye, until the pupil becomes extremely small; it will then be observed, that the circle of the pupil is first contracted—that the radial fibres again dilate it, but so slightly that the first motion was the most effectual; and this alternation continues until contraction of the pupil is accomplished. When the light is withdrawn, its stimulus ceasing to affect the circular fibres, the radial contract and increase the aperture; but the circular now take on a counter action in the same proportion as the radial did before, so that the pupil is again diminished; these motions continue until the pupil has gained its former size. These phenomena prove indisputably, that if there are circular fibres, light is their direct stimulus, affecting them alone; light does not then relax the opponent muscle, for it continues to contract against the force of the other, which force seems its proper stimulant, and which only yields to a strong light, not to the light of day. The latter keeps the circular fibres in a perpetual and almost invisible motion, which motion, pulling on the radial fibres, keeps them greatly contracted. Hence,

1. Light is the stimulus which causes the orbicular fibres to contract.

2. Contraction of the orbicular fibres brings the radial fibres into a state of tension, which tension is equivalent to a direct stimulus, and causes their contraction.

3. Tension is a stronger stimulant to the radial, than the diluted light of day is to the orbicular fibres, but weaker than the light of the sun, of a lamp, &c.

T. G. Hake.

Glasgow, Dec. 25th, 1830.

TREATMENT OF CHRONIC ENLARGEMENT OF THE TONSILS WITH IODINE.

By G. Smith, *M.R.C.S., &c. &c.*

In a late number of The Lancet I inserted the results of a case of ranula which I successfully treated with iodine tinct., to which case I was induced to give publicity before the cure was completed, that others who should meet with this disease might give this medicine a fair trial in preference to the usual modes of operating, which have with few exceptions been not only painful and hazardous, but uncertain in their results. Finding that the iodine in this case acted so powerfully, and with such certainty, I decided on trying its merits in other cases of a similar character, on the first favourable opportunity which should occur in practice. The following is a case of congenital enlargement of the tonsil glands, with the effect of iodine, as inserted in my case-book:—

Master G., aged six, was brought to my surgery February 26th last; complained of no pain, but had great difficulty of breathing, particularly in the night when asleep; each inspiration was performed with a laborious effort in the respiratory muscles, and from the alternate hurried and oppressive breathings sometimes a considerable cessation was produced; the parents of the boy were only anxious to learn if the disease was likely to prove suddenly fatal, as they did not flatter themselves that any-thing could be done to alleviate his sufferings, much less did they anticipate a cure. His difficulty of breathing was first observable when four months old, gradually becoming more troublesome, but within the last two or three years it was quite alarming; the parents of the boy had applied to every source for relief where there was the most remote chance, but all refused to interfere in a case which had always been treated by operation, and that operation uncertain in its results, with the exception of one of our *old ladies*, who promptly applied a blister all over the chest, and pronounced the disease consumption. From the sonorous breathings, which could be heard at a considerable distance, I examined his throat, and could easily perceive an unnatural enlargement of the tonsil glands, which were considerably larger than is generally the case in cynanche tonsillaris; they occupied the whole of the posterior arch, pressing upwards the velum pendulum palati, exciting frequent retching, with a suffocating feeling from the uvula passing behind the tonsils in inspiration, and requiring a convulsive effort to force it anterior to them. Such was the miserable situation of my young patient when first presented for examination.

Not willing to lose time in a case which even an attack of simple catarrh might render fatal, I explained to them the necessity of prompt interference in a case so critical, and instantly proposed excision or ligature as the only remaining chance; the friends were, however, averse to any manual interference, supposing that from his great weakness he could not rally after an operation. Recollecting the great influence which I had repeatedly witnessed to be exerted by iodine, I immediately ordered eight drops of the tincture (the strength of tincture I use is in the proportion of thirty-six grains of iodine to one ounce of rectified spirit; macerate for fourteen days and strain) twice a day in a little sugar and water, with directions to increase the dose two drops every second day, and carefully to observe what effect the increased dose produced; when we had reached twenty-four drops three times a day, the patient complained of swimming in the head, stupor, loss of recollection, sickness, vomiting, and bloody stools; these symptoms considerably alarmed the parents, but they were soon relieved by an emetic followed up with the warm bath and a laxative; that the stomach might regain its usual tone I suspended the medicine for two or three days, and recommenced with only twelve drops (it may be noticed that if this medicine is suspended for any length of time, it is not safe to resume the dose that was given before the interval of suspension) three times a day, gradually increasing till we reached twenty-four drops, the former dose; the effect was not so alarming as before, but I found this was the maximum dose, from a partial recurrence of the former symptoms. The first change which was effected, as in my former case, was a general improvement in the system, which partially convinced me that its effect might be salutary to some extent, and that by steady continuance in its use more would be effected than I had at first anticipated. Watching narrowly from time to time for a lessening of the general structure of the gland, I was not a little surprised on the tenth week of the medicine to see on the anterior part of the right tonsil four small openings, which I concluded were the excretory ducts considerably dilated; this no doubt was the effect of the medicine, although operating very differently from what I expected; the glands appeared to be affected very peculiarly both in colour and alteration of surface; the openings gradually enlarged till they attained the astonishing size of two lines in diameter; with great caution I introduced my probe into each opening, and could easily feel several small gritty substances, which on extraction I found to be small calculi; at different times I extracted ten in all, weighing on an average three and a half grains each. I washed the cavity with warm water, and gave strict orders that nothing but fluids should pass into the stomach.

I am utterly unable to account for the peculiar way in which in this case the iodine acted, otherwise than by supposing that its influence on the glandular system, &c., was so great that there was a considerable absorption of the natural structure of the gland, which continuing to act pressed on the contained substance or calculi, and these no doubt found an easier exit at the excretory duct than any-where else.

I gave the iodine a fair trial in two cases of chronic enlargement of the testicle without any decided advantage.

Barnsley, Yorkshire, Dec. 24, 1830.

CHOLERA MORBUS IN RUSSIA.

[From a Correspondent.]

It is said to be in contemplation to send a medical deputation from England to Russia, to investigate the nature of the epidemic prevailing in that country; if so, we trust that the individuals composing it will be selected with judgment and impartiality, and not like Gibraltar, left to those who have an object in perpetuating the doctrines of contagion and non-liability to second attacks. Should it be intended to make it a military medical deputation, and volunteers from the half-pay of the medical department are not procured, as it will be a service not only of severity but danger, it is to be hoped that the duty will be given to some of the accomplished and experienced officers who have been for many years luxuriating on the wealth bestowed upon them by the public for services, the importance of which were hithorto unknown to the officers of the department generally. Amongst the number may be mentioned Sir James Fellows, Sir James R. Grant, Dr. Frank, inspectors of hospitals, and Dr. Somerville (formerly a member of the Army Medical Board), a name first in importance, and it may be added, the greatest in military medical jobbing, who is at present in possession of a house from the public, and 500*l.* per annum, at Chelsea, and of 600*l.* per annum, as a retired member of the Army Medical Board; if this great, learned, and scientific doctor, in addition to his many accomplishments, is endowed with ordinary feelings, and should be asked for what services to his country he is thus remunerated, he would blush on recounting them; yet, if reports are correct, these are the least part of his spoils, his loaves and fishes at the Cape of Good Hope and in Canada are unnumbered, but of which we may be able to give some account at a future period. It would be well if the director-general, whose propensities for jobbing are rather suspected, would take a hint, if it is not already too late, as it is said a view of the department is soon likely to be submitted to the public exposing the extraordinary chaos into which, either from ignorance or motives more unpardonable, he has plunged it. Such a state of things is, perhaps, unprecedented in any organised department.

We observe that by accounts from Berlin, dated December 24, the Minister of Science, of Instruction, and of Public Health, has directed four physicians to proceed to Moscow to study the *cholera morbus* in all its stages. Dr. Daune has already set out for St. Petersburgh; he will be followed by Drs. Quincke and Barchemtsch. The chief of the Physicians, Abert de Gumbennen, is to put himself at the head of this deputation.

ALLEGED CLANDESTINE POST-MORTEM EXAMINATION.

To the Editor of The Lancet.

Sir,—Many thanks for your exertions o behalf of the medical pupils of this metropolis; they have incurred a deal of gratitude to you, which they will not very easily pay. But with all your exertions, you have not been able to instil into the minds of *all* our teachers, those principles of honour which ought to guide the whole of their conduct: witness the following circumstance which has lately occurred.—Having attended Mr. Waller's excellent practical lectures on midwifery at the School in Aldersgate Street, I afterwards attended the practice of the London and Southwark Midwifery Institution. During this time, it fell to my lot to attend a poor woman who was delivered of a child with a very large tumour joining from the back of the head. The child died, and I was anxious to have the advantage of an inspection. My preceptor Mr. Waller, and likewise Mr. Tyrrell, who had previously seen the case, both wished for the examination: whilst we were endeavouring to obtain permission, judge of my surprise at hearing that Mr. Stanley, of St. Bartholomew's Hospital, unknown to any of us, had been to the house of our patient with some of his dressers, and had actually the impertinence to propose an examination to be conducted by himself; thus endeavouring to deprive us, who had been anxiously watching the case, and to whom it properly belonged, of the advantage we had a right to expect in preference to any one else. I sincerely hope such a violation of professional etiquette does not often occur. Since there is no tribunal for the punishment of conduct of this kind, the only chance of obtaining redress is to lay the grievance before the public.

I remain yours respectfully,

A Pupil of Mr. Waller.

TO CORRESPONDENTS.

Medea. Should a reply to the paper inserted this week be deemed necessary, may we request that *brevity* be adopted as a motto.

Q. asks, "Is it proper that poor patients should be sent from St. George's Hospital with their *eyes cut out?*"